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THE METHOD OF CONSTANT STIMULI AND ITS GENERALIZATIONS.[1]

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We introduced in several previous publications[2] the notion of the probability of a judgment, which is the fundamental notion in the analysis of psychophysical measurement methods. The judgments of a subject who compares two stimuli under well-defined and constant conditions, have the formal and material character of those chance events which are spoken of in the calculus of probabilities. These probabilities are *a priori* entirely unknown and must be determined by observation. Experience shows that if a standard stimulus of given intensity is compared under constant conditions with stimuli of varying intensities, the probabilities of the different judgments vary in a certain way with the intensity of the comparison stimulus. Let the subject be required to express his judgment in either one of the terms smaller, equal or greater. The probability of the judgment greater increases and that of the judgment smaller decreases with increasing intensity of the comparison stimulus, whereas the probability of an equality judgment first increases and then decreases after having reached a certain maximum.

[1] An abstract of this paper, but without the tables, was presented at the spring meeting of the experimental psychologists at Harvard in 1908. A full presentation of this topic with all the necessary demonstrations was given in the treatise on 'Die psychophysischen Massmethoden als Grundlagen empirischer Messungen' in Vols. 15 and 16 of the *Archiv f. d. ges. Psychologie.*

[2] *The Application of Statistical Methods to the Problems of Psychophysics,* Philadelphia, 1908; 'On the Method of Just Perceptible Differences,' PSYCHOLOGICAL REVIEW, 1907, Vol. 14, pp. 244–253; 'Die psychophysischen Massmethoden als Grundlagen empirischer Messungen,' *Archiv f. d. ges. Psychologie,* Vols. 15 and 16; also in the report on 'Die Psychologie in Amerika,' *Archiv f. d. ges. Psychologie,* 1908, Vol. 11, 'Literaturbericht,' pp. 141–143.

This suggests the view that the probabilities of these judgments are functions (in the mathematical sense of the term) of the intensity of the comparison stimulus. A mathematical expression which gives the probability of a judgment as function of the comparison stimulus, is called the psychometric function of this judgment. These expressions are, of course, different for different intensities of the standard stimulus. It is not necessary but perhaps advisable to insist on the fact that such an expression refers only to well-defined experimental conditions, because it is not possible to assign definite values to these probabilities unless one refers to definite physical and psychophysical conditions under which the observations are made. The term psychometric function was chosen in imitation of the term biometric function, which is commonly in use for mathematical expressions which give the so-called probability of dying as function of age. If the psychometric functions of all the judgments are known, one is able to predict the outcome of any set of experiments.

For the practical application of this notion not only the form of these functions but also the values of all their constants must be known or data must be at hand by which they can be determined. Our choice of the expressions which may represent the probabilities of the different judgments as functions of the intensity of the comparison stimulus must be guided by the following consideration: Experience shows that the comparison of a standard with much greater intensities results always, or nearly always, in the judgment greater, whereas the judgment smaller is given exclusively, or almost exclusively, on the comparison with very small intensities. None of the functions, furthermore, must assume values greater than one and smaller than zero, because they represent mathematical probabilities. From this it follows that the psychometric functions of the greater and of the smaller judgments approach the values zero and one asymptotically, the psychometric function of the smaller judgments decreasing and that of the greater judgments increasing with growing intensities of the comparison stimulus. The psychometric function of the equality cases has a certain maximum, on both sides of which it falls off steadily and approaches

the value zero asymptotically. Add to this the condition that for any given intensity of the comparison stimulus the sum of all the psychometric functions must be equal to one. These conditions, however, are not sufficient to determine the nature of these functions and one must form a hypothesis about them, which may be based either on experience or on theoretical considerations. The number of psychometric functions in any set of experiments is equal to the number of judgments admitted. There are always two functions, the psychometric functions of which are similar to those of the smaller and greater judgments; judgments of this type are called extreme judgments. Judgments the psychometric function of which is similar in its course to that of the equality judgments may be called middle judgments. The number of middle judgments is always uneven; until now one has not found it necessary to go beyond the number of three middle judgments.

There is no special difficulty in devising algebraic expressions which may serve as hypotheses on the psychometric functions. A criterion of the value of different hypotheses can consist only in the greater or smaller agreement with experience, which is measured by the sum of the squares of the deviations of the observed from the calculated values. A mathematical expression, which may serve as a hypothesis on these functions, depends on a number of constants which must be determined by observation. These observations consist of empirical determinations of unknown probabilities and are, as such, subjected to errors, which prevent us from determining the exact values of the constants. This must cause differences between the true and the calculated values even if our hypothesis on the nature of the psychometric functions is correct. We will speak in this case of a lack of agreement between theory and observation on account of errors of observation. But it also is possible that our hypothesis on the psychometric functions is incorrect and in this case there must be a lack of agreement between theory and observation, even if all the constants were absolutely exact. Errors which are due to an incorrect hypothesis on the psychometric functions may be called errors of theory. It is not easy to decide whether in a certain case we have to deal with errors

of observation or with an error of theory, because, errors of observation being inevitable, errors of theory are always intermingled with them. Usually, however, this problem will be put in this way: that it is required to decide which one of a certain group of functions is best suited to represent a given experimental material. The errors of observation in this case remain the same for the different hypotheses and the sums of the squares of the deviations of the calculated from the observed values indicate the greater or smaller agreement of a hypothesis with experience.

One may try to get along without making a definitive hypothesis on the nature of the psychometric functions and, starting from the theorems that every function may be represented by a power series, one may determine from the data of observation as many coefficients as possible. If n comparison stimuli were used in the experiments one may determine n constants, by which all the terms up to that of degree $(n - 1)$ are determined. This representation of the data of observation were absolutely exact, if the psychometric functions could be represented by an algebraic equation of degree $(n - 1)$, but even if this is not the case this method is distinguished in so far as it requires the smallest amount of theoretical additions to make a mathematical treatment of the data possible. The degree of the equations which represent the psychometric functions depends on the number of observations and is different for different sets of experiments, unless it happens that the same number of comparison stimuli had been used. In this sense one may say that the representation of the data by means of an algebraic equation does not involve a definitive hypothesis on the psychometric functions at all. It is easy to see, however, that this method can not possibly give a definitive result. The psychometric functions represent mathematical probabilities and are, as such, restricted to the interval from zero to one, whereas an algebraic function exceeds every limit for sufficiently large values of the independent variable. Lagrange's formula of interpolation and Newton's method of differences are the most convenient ways of treating the data according to this hypothesis and of determining new values of the psychometric functions

without actually setting them up. We will illustrate this method by working out a set of results which has served for the illustration of the method of just perceptible differences.[1]

TABLE I.

RELATIVE FREQUENCIES OF THE JUDGMENTS GREATER, SMALLER AND EQUAL.

Comparison Stimulus.	Equal.	Greater.	Smaller.
84	0.0444	0.0222	0.9333
88	0.1133	0.0244	0.8622
92	0.1889	0.1111	0.7000
96	0.2578	0.2933	0.4489
100	0.2400	0.5289	0.2311
104	0.0889	0.8156	0.0956
108	0.0800	0.9044	0.0156

Let us suppose we made observations with n comparison stimuli which we call $x_1, x_2, \cdots x_n$ and that these comparison stimuli gave to a certain judgment the probabilities $a_1, a_2, \cdots a_n$. Lagrange's formula then has the form

$$F(x) = \frac{(x-x_2)(x-x_3)\cdots(x-x_n)}{(x_1-x_2)(x_1-x_3)\cdots(x_1-x_n)}a_1 + \frac{(x-x_1)(x-x_3)\cdots(x-x_n)}{(x_2-x_1)(x_2-x_3)\cdots(x_2-x_n)}a_2 + \cdots + \frac{(x-x_1)(x-x_2)\cdots(x-x_{n-1})}{(x_n-x_1)(x_n-x_2)\cdots(x_n-x_{n-1})}a_n.$$

The actual setting up of the equation is laborious but not necessary for interpolating new values of the function. It is a matter of course that one must arrange the computation systematically whenever one has to treat an extended experimental material. The circumstance that the sum of all psychometric functions for any intensity of the comparison stimulus must be equal to one, shortens the work considerably. If three judgments were admitted it is sufficient to calculate by Lagrange's formula the probabilities for two judgments only, that of the third being found by subtracting the sum of the other

[1] PSYCHOLOGICAL REVIEW, Vol. 14, p. 249, 251. These results are taken from subject II, in the experiments on lifted weights, cf., 'The Application of Statistical Methods to the Problems of Psychophysics,' pp. 178, 217, and *Arch. f. d. ges. Psychologie*, Vol. 15, p. 287.

two from the unit. This is the way in which the interpolated values in Table II. were found. From the data of such a table a diagram may be constructed which shows graphically how the probabilities of the judgments vary with the intensity of the comparison stimulus. Such a diagram is shown in Chart 1. The intensities of the comparison stimulus are represented on the abscissa and the corresponding values of the psychometric functions on the ordinate. It is necessary to draw the two axes on different scales; in our drawing the unit of measurement of the y-axis is ten times as large as that of the x-axis.

TABLE II.

COMPARE CHART 1.

Comparison Stimulus.	Equal.	Smaller.	Greater.
84	0.0444	0.9333	0.0222
85	0.0614	0.9101	0.0285
86	0.0786	0.8956	0.0258
87	0.0959	0.8814	0.0227
88	0.1133	0.8623	0.0244
89	0.1310	0.8353	0.0337
90	0.1497	0.7987	0.0516
91	0.1690	0.7533	0.0777
92	0.1889	0.7000	0.1111
93	0.2088	0.6407	0.1505
94	0.2279	0.5775	0.1946
95	0.2447	0.5129	0.2424
96	0.2578	0.4489	0.2933
97	0.2654	0.3875	0.3471
98	0.2659	0.3301	0.4040
99	0.2578	0.2779	0.4643
100	0.2400	0.2311	0.5289
101	0.2125	0.1900	0.5975
102	0.1761	0.1541	0.6698
103	0.1334	0.1230	0.7436
104	0.0889	0.0955	0.8156
105	0.0495	0.0710	0.8795
106	0.0251	0.0494	0.9255
107	0.0294	0.0303	0.9403
108	0.0800	0.0156	0.9044

There are two difficulties in treating the data by Lagrange's formula of interpolation. It happens in many cases that values are obtained which are greater than one or smaller than zero. The occurrence of these impossible values can have symptomatic value only. Their presence is easily seen in a graphic representation of the functions, because in these places the

curves rise above a line drawn parallel to the x-axis at the distance unity, or fall beneath the abscissa. The second difficulty is that the course of the functions is irregular near the beginning and the end of the tables, the psychometric functions of the greater judgments not increasing, and those of the smaller judgments not decreasing throughout the whole interval.

In spite of these irregularities, however, there is in the data of all the subjects an interval, which lies in the central parts of the tables, inside of which the psychometric functions behave regularly and are not interrupted in their course by any irregularities. The intensities for which the psychometric functions of the extreme judgments assume the value 1/2 are found inside of these intervals. Between these two stimuli lie all the intensities which do not give a probability equal to or exceeding the value one half to either one of the extreme judgments. For this reason it is called the interval of uncertainty. This interval is determined by the method of just perceptible differences and the comparison of the accuracy of sense perception of different subjects, or of the same subject at different times or under different conditions is based on it. It is easy to determine this quantity by interpolation. In determining the lower limit of the interval of uncertainty one picks out the two stimuli which gave to the judgment smaller probabilities just above and just below one half and interpolates new values in this interval by Lagrange's formula, until the quantity to be determined is included in an interval small enough to admit of an interpolation on a straight line. Owing to the regularity of the course of the psychometric functions in the middle of the table only few interpolations by Lagrange's formula are needed for this calculation. We give here a table of the results of the determination

Subject.	Lower Limit of Interval of Uncertainty.	Upper Limit of Interval of Uncertainty.	Length of Interval of Uncertainty.
I.	93.26	100.95	7.69
II.	95.20	99.55	4.33
III.	98.65	100.32	1.57
IV.	95.24	98.26	3.02
V.	93.75	95.83	2.08
VI.	95.82	101.04	5.22
VII.	95.33	100.74	5.44

of the interval of uncertainty by Lagrange's formula for all the persons who served as subjects in our experiments on lifted weights.

The interval of uncertainty is different for different subjects. If we order our subjects by the length of this interval every subject will have a definite place in the series, except when two or more individuals happen to have intervals of uncertainty of the same length. In this latter case the subjects are equally sensitive.

We now turn to the study of the psychometric functions of the equality cases. It is interesting to determine the intensity of the comparison stimulus for which the probability of an equality judgment is greatest. This value can be found easily by means of Table II. We pick out the three greatest values of the table; these values, which we call A, B, C, may correspond to the intensities x_{R-1}, x_R, x_{R+1}, of the comparison stimulus. The maximum of the probability of the equality judgments is found at the point $x_R + \xi$ where the quantity ξ is given by the expression

$$\xi = \frac{A + C}{2\,(A + C - 2\,B)}\,.$$

The maximum probabilities of the equality judgments are found for our seven subjects at the intensities 98.61, 97.57, 100.51, 97.34, 95.89, 99.23 and 96.80. The maximum probability is attained for intensities which are greater than the upper limit of the interval of uncertainty in two cases (subjects III. and V.), from which we conclude that the situation of this maximum has not a definite relation to the interval of uncertainty. It may be remarked that a similar conclusion may be drawn in respect to the arithmetic mean of the equality cases, which represents the abscissa of the center of gravity of the area included between the curve representing the psychometric function of the equality cases and the abscissa.

It is of greater consequence to consider the maximum values of the psychometric functions of the equality judgments. These quantities are found by introducing into Lagrange's formula the values for which the maximum is attained. The

results of this calculation for our seven subjects are 0.4860, 0.2667, 0.1508, 0.2111, 0.1969, 0.3771, 0.3797. These numbers show that the probabilities of the equality judgments are not great; in no case do they exceed one-half and only in the case of subject I does this probability come anywhere near this value, while it never exceeds 0.38 for anyone of the other subjects. This shows that we cannot speak of a point of equality in any absolute sense of the word, because we have to require that such a point must give to the equality judgments probabilities greater than one-half.

Let us compare the orders which we obtain when the subjects are ordered by the length of their intervals of uncertainty and by the maximum probabilities which they give to equality judgments. We obtain the following arrangements of our seven subjects.

Subjects Ordered by Maximum Probability of Equality Judgments.	Subjects Ordered by Length of Interval of Uncertainty.
I.	I.
VII.	VII.
VI.	VI.
II.	II.
IV.	IV.
V.	V.
III.	III.

The order of all the subjects is the same in both cases, and we conclude from this fact that the maximum probability of the equality judgments can be used just as well as a measure of the accuracy of sense perception as the interval of uncertainty. It is, therefore, possible to base a comparison of the accuracy of sense perception on the equality cases alone, a fact which may be surprising if one remembers that these judgments are so much of a difficulty in the customary treatment of psychophysical measurement methods that some investigators advised the absolute suppression of these judgments.

The fact that there exist two quantities each one of which may be used for the purpose of comparing the accuracy of sense perception is interesting, because these quantities are independent of one another and are derived from different data. The limits of the interval of uncertainty depend directly only

on the probabilities of one of the extreme judgments, on those of the equality judgments and of the other extreme judgment only indirectly through the sum of the probabilities which must be equal to one. The probabilities of the equality judgments have, therefore, no direct influence at all on the length of the interval of uncertainty. The maximum probability of the equality cases, on the other hand, is entirely independent from the probabilities of the extreme judgments, and the methods of calculation, which lead to the determination of the maximum probability of the equality judgments and of the interval of uncertainty, have nothing in common. We will conclude from this fact that there exist certain relations between the different psychometric functions, which must be investigated.

It is not possible to base the definition of the point of subjective equality on the equality judgments. There remains the possibility of basing this definition on the extreme judgments by defining the point of subjective equality as that intensity of the comparison stimulus for which the probabilities of the extreme judgments are equal. If r, s, t are the probabilities of the smaller, equal and greater judgments, this definition of the point of subjective equality implies that $r = t$; no specification is made in regard to the values of either one of these quantities. The approximate determination of this point from a table of the psychometric functions offers no difficulty. We find in the table the stimulus x_1 which gives a higher probability to the smaller than to the greater judgments and which is immediately followed by the stimulus x_2 which gives a higher probability to the greater than to the smaller judgments. Let the corresponding probabilities of the smaller judgments be y_1, y_2 and those of the greater judgments z_1, z_2. If the interval between the points x_1 and x_2 is small enough to admit of an interpolation on a straight line the abscissa of the point of intersection of the curves representing the psychometric functions of the extreme judgments is given by the formula

$$x - x_1 = \frac{(x_1 - y_1)(x_2 - x_1)}{y_2 - y_1 + z_1 - z_2}.$$

We give here the results of this calculation for the seven sub-

jects of our experiments on lifted weights with reference to the limits of the interval of uncertainty.

Subject.	Lower Limit of Interval of Uncertainty.	Point of Subjective Equality.	Upper Limit of Interval of Uncertainty.
I.	93.26	97.20	100.95
II.	95.20	97.36	99.55
III.	98.65	99.46	100.32
IV.	95.24	96.62	98.26
V.	93.75	94.66	95.83
VI.	95.82	98.42	101.04
VII.	95.33	98.59	100.74

These numbers show that the point of subjective equality defined in terms of the probabilities of the extreme judgments lies always inside of the interval of uncertainty and a closer inspection reveals the fact that it coincides very closely with the centre of this interval. Let us divide the distance of the point of subjective equality from the lower limit of the interval of uncertainty by the length of this interval. We obtain the following values for seven subjects: 0.49, 0.50, 0.51, 0.54, 0.56, 0.50 and 0.40, the average of which is 0.50. We conclude from this, that the point of subjective equality coincides with the center of the interval of uncertainty.[1] The difference between the point of subjective equality and the standard gives the constant error which in the case of our experiments is due to the order in which the weights were presented to the subject, *i. e.*, to the so-called time error. We find the following values

[1] This proposition is the justification of all the investigations in which equal appearing stimuli are found by the method of just perceptible differences, as, *e. g.*, in Miss Cook's investigation on the illusion of filled and unfilled tactual spaces. Only the just imperceptible positive and negative differences were determined in her experiments (the just perceptible differences were used only in some series) and the averages of these values were taken. This average determines the center of the interval of uncertainty, *i. e.*, the point of subjective equality which of course has to be determined in a study of an illusion. Miss Cook, however, did not use the method of just perceptible differences in its traditional form but in Sanford's variation, so that one can not make the positive statement that she really determined the stimulus which gives equal probabilities to the extreme judgments. It is not likely that the influence of her variation on the final result is great, but it is inconvenient to have to deal with results which escape an exact interpretation. Cf. Helen Dodd Cook, 'Die taktile Schätzung von ausgefüllten und leeren Strecken,' *Archiv f. d. ges. Psychologie*, 1910, Vol. 16, pp. 451-456.

for this quantity 2.80, 2.65, 0.54, 3.38, 5.34, 1.58 and 1.41. All these differences being negative we clearly have to deal with an over-estimation of the second weight.

An extremely important conclusion may be drawn from this proposition. The similarity of the method of just perceptible differences with the procedure by which we determine unknown empirical quantities by measurement was noticed by several investigators, although one could not agree as to the exact point of similarity. An accurate understanding of the method of just perceptible differences is of the greatest importance for the general theory of empirical measurements. In determining the unknown weight of a body, for example, we start after a preliminary rough determination of the approximate value by comparing the body with known weights which are too great and gradually reduce the difference until no difference is noticed between the two weights. We keep on diminishing the comparison weight until a point is found where the comparison weight is found to be too small. The average of these results is taken as a determination of the unknown weight of the body. The whole process is repeated in the ascending direction, which gives another determination. Several such determinations have to be made for the purpose of obtaining an exact determination and the final result is found by the arithmetic mean of all the individual results.

The fact that we pick out the arithmetic mean as the final determination of the quantity to be measured implies that we regard it as the best value obtainable. This is the meaning of the proposition that the arithmetic mean has to be regarded as the most probable value of a set of measurements of an empirical quantity. Taking this proposition as a principle one may deduce from it the method of least squares, which is a set of rules for finding the most probable values of empirical quantities and the limits of the exactitude of their determination, if the principle of the arithmetic mean is granted. This is the way of reasoning followed by Gauss in his first deduction of the method of least squares. The theory of errors of observation based on this principle has stood the test of practice for more than a hundred years, and it may be granted that the principle of the

arithmetic mean seems to be a very obvious one, but it can not be denied that it makes the impression of being artificial. A great number of attempts were made to demonstrate it, but no proof could be given without introducing some other proposition which is equivalent to the principle of the arithmetic mean. As it was known that other definitions of the most probable value of a set of measurements of an empirical quantity lead to entirely different rules of calculation, one began to suspect that the theory of errors of observation required the introduction of a principle of empirical origin, just as much as the use of ordinary geometry in geodesy and astronomy requires the proposition that empirical space is a three-dimensional Euclidean manifoldness.

It is only a step in the same direction of development to prefer an empirical verification of the law of distribution, which follows from the principle of the arithmetic mean, to any *a priori* deduction, because such a demonstration must necessarily start from some other hypothesis. This is the view expressed by H. E. Faye and H. Laurent. For those who maintained this view it became necessary to show that the distribution of the individual results in a set of measurements is such as to warrant the application of the principle of the arithmetic mean. This required observations on actual distributions of errors, which were given in the works of Bessel, C. S. Peirce, Guarducci, Laurent, Helmert, F. Y. Edgeworth and others. In the investigations referred to a satisfactory agreement between theory and practice was observed, but this position became seriously endangered, not to say untenable, by the discovery that most empirical distributions show an essential asymmetry, symmetry being found as an exception only in very rare cases. One might suspect that the symmetry observed in sets of measurements is merely a chance result or the effect of some peculiar conditions, as it very likely is the case of Peirce's observations.[1]

[1] C. S. Peirce, 'On the Theory of Errors of Observation,' Report of the U. S. Coast and Geodetic Survey, 1870, made a very extended series of observations on reaction time using the Hipp chronoscope. His results, curiously enough, show a symmetrical distribution, although it is one of the best established facts that the distribution of reaction times is asymmetrical. It is of course out of question that Peirce did not observe or report correctly and it is all the more

But granting even that there is no flaw in any of these observations, one must ask how it comes that the errors of observations show a symmetrical distribution while all the other empirical distributions are essentially and normally asymmetrical. Neither an empirical nor a mathematical justification of the principle of the arithmetic mean is at hand and the method of least squares seems to hang in the air. We are confronted with the shocking situation that a proposition is triumphantly borne out by an immense indirect experience and that it can be proved neither by mathematical deduction nor by direct experience.

The only case where there does not exist a doubt as to the justification of the principle of the arithmetic mean is that of the empirical determination of unknown probabilities, because the arithmetic mean is the most probable value of the quantity to be determined according to the theorems of the calculus of probabilities. This fact has to be utilized for the theory of errors of observation. A single determination of an empirical quantity is obtained by following a strict rule which determines which intensity has to be put down as the result of an individual measurement. On the basis of any such definition one can set up an expression which has the character of a mathematical expectation and it follows that the most probable value of a set of individual determinations is given by their arithmetic mean. The arithmetic mean, therefore, is the most probable value in all those measurements in which a systematic procedure is strictly followed.

In those cases where the individual determinations were obtained by following the procedure described above we can go a step further. The first result of such a determination is what we call the just imperceptible positive difference, and the point where the balance indicates a difference between the two bodies is the just perceptible negative difference. In the ascending series we determine first the just imperceptible negative, and then the just perceptible positive difference. Since

necessary to explain his result, because Pizzetti, 'Ifondamenti matematici per la critica dei risultati sperimentali,' 1892, attributes great weight to Peirce's observations. The symmetry of the distribution in Peirce's results is due to the mixture of different distributions, as will be shown in a paper to be published in the near future.

the final average of the set is not influenced by grouping them and taking the arithmetic mean of the averages of the groups we may combine first the just perceptible and the just imperceptible positive, and then the just perceptible and the just imperceptible negative differences, obtaining in the first case the threshold in the direction of increase and in the second the threshold in the direction of decrease. The arithmetic mean of these two quantities is identical with the average of all the individual results, from which it follows that the final determination of the value of an empirical quantity coincides with the center of the interval of uncertainty. We conclude that we determine by our empirical measurements those intensities for which the probability of the judgment greater is equal to that of the judgment smaller.

We thus obtain the remarkable result that the foundations of the theory of errors of observation are found in the theory of psychophysical measurement. The principle of the arithmetical mean as the most probable value of a set of empirical measurements since more than a hundred years proved refractory to all attempts at a purely mathematical demonstration and empirical demonstrations lack finality because they do not show the cause of the symmetry of this distribution in the face of an indefinite number of asymmetrical distributions. The cause of this failure is to be sought in the notion of the probability of an error of certain size, which is the basis of the theory of errors of observations. The process by which we arrive at assigning a definite value to an empirical quantity is very complicated and requires further analysis. This analysis, however, can be given only by means of the notion of the probability of a judgment, which is entirely foreign to the theory of observations, because this science considers only the result of the process of measuring. The notion of the probability of a judgment belongs to psychology as well as the analysis of the conditions which influence it. It was assumed in most, not to say in all the treatises on the theory of psychophysical measurement that this science has to depend on the theory of errors of observations, which furnishes the data on which we have to build. This is not the case. The relation of these two sciences is just the opposite. Psychology furnishes the notion of the probability of a judg-

ment and thus opens the way to an understanding of the principle of the arithmetical mean, and it also furnishes the notion of the accuracy of sense perception. The theory of errors of observation reciprocates by offering problems and refined observations, which properly belong to the psychology of sense perception.

We now turn to the study of the treatment of the psychometric functions by means of definitive hypotheses. Let $f(x)$, $g(x)$, $h(x)$ be the functions which represent the smaller, equal and greater judgments. Each one of these functions also depends on a number of parameters which must be determined from the data of observation, so that we may write explicitly $f(x;\ a_1, b_1, c_1, \cdots)$, $g(x;\ a_2, b_2, c_2, \cdots)$, $h(x;\ a_3, b_3, c_3, \cdots)$. Generally the number of observations is greater than the number of constants to be determined, so that they must be determined from an overdetermined set of equations. Owing to the fact that empirical determinations of unknown probabilities are not exact, certain discrepancies between the different results will arise which must be eliminated by an adjustment. The procedure to be used for this purpose will become clear by the following considerations. The individual observations consist of empirical determinations of certain unknown probabilities, the probable errors of which are given by the theorem of Bernoulli. Let s_R experiments be made with the comparison stimulus x_R, m_R of which may have resulted in the judgment smaller, o_R in the judgment equal and the rest $n_R = s_R - (m_R + o_R)$ in the judgment greater. The fractions m_R/s_R, o_R/s_R, n_R/s_R are the most probable determinations of the underlying probabilities and the probability that these results are affected by an error of the size γ is given by the expression

$$\frac{h}{\sqrt{\pi}} e^{-h^2\gamma^2},$$

where h, the coefficient of precision, is given for the three probabilities by

$$\sqrt{\frac{s_R^3}{2m_R(s_R - m_R)}},\quad \sqrt{\frac{s_R^3}{2o_R(s_R - o_R)}},\quad \sqrt{\frac{s_R^3}{2n_R(s_R - n_R)}}$$

respectively. The results of the experiments with every comparison stimulus, therefore, give three equations of the form

$$f(x_R;\ a_1, b_1, c_1, \cdots) = \frac{m_R}{s_R},$$

$$g(x_R;\ a_2, b_2, c_2, \cdots) = \frac{o_R}{s_R},$$

$$h(x_R;\ a_3, b_3, c_3, \cdots) = \frac{n_R}{s_R},$$

which must satisfy the condition equation

$$f(x) + g(x) + h(x) = 1.$$

One might believe that a condition equation must be entered for every observation, but this is not the case, because the sum of all the psychometric functions must be equal to one for any value of the comparison stimulus, from which it follows that this sum must be identically one. This simplifies the calculation considerably, because one of the psychometric functions may be determined as the difference of one minus the sum of the two other functions, so that the adjustment of the observations need be carried through only for two of them.

Let us suppose that $g(x)$ is determined by $f(x)$ and $h(x)$. We then have only two observations for every stimulus, for which the probabilities of the different judgments were observed. This gives a system of equations for the determination of the quantities $a_1, b_1, c_1, \cdots$ which occur only in the equations originating from $f(x)$, and $a_3, b_3, c_3, \cdots$ occurring only in the equation originating from $h(x)$. The whole system may be solved by treating these two groups separately, and since the constitution of both groups is the same, we may confine the theory to showing how one of them may be solved. Let us take the group which contains the constant of $f(x)$ and refer to them by the letters $a, b, c, \cdots$, omitting the indices.

We introduce here for the sake of simplifying the calculation the assumption that the constants $a, b, c, \cdots$ occur in $f(x)$ in such a way as to form a linear complex

$$\alpha_R a + \beta_R b + \cdots$$

where the quantities $a_R, \beta_R, \cdots$ depend on the intensities of the comparison stimulus and are, therefore, different for different intensities. We also assume that there exists an inverse function $F(x)$ so that

$$a_R a + \beta_R b + \cdots = F\left(\frac{m_R}{s_R}\right).$$

We obtain under these conditions an overdetermined system of linear equations the errors in the determinations of the constants of which follow the exponential law. From this it follows that the most probable values of the unknown quantities must be found by the method of least squares, each equation being put down with the proper weight. Only the fractions $p_R = m_R/s_R$ are directly determined, their coefficient of precision h_R being given by the theorem of Bernoulli. The coefficient of precision in the determination of $F(p_R)$ may be called H_R and is found by the formula

$$\frac{1}{H_R^2} = \frac{\left(\frac{dF}{dp}\right)_{p=p_R}}{h_R^2}.$$

Since the weight of every observation equation is directly proportional to the square of the coefficient of precision in the determinations of $F(p)$, we have all the quantities which we need for the solution of our system which has this form

$$a_1 a + \beta_1 b + \cdots = F_{(p_1)} \text{ with the weight } P_1,$$
$$a_2 a + \beta_2 b + \cdots = F_{(p_2)} \quad " \quad " \quad " \quad P_2,$$
$$\cdots\cdots\cdots$$
$$a_l a + \beta_l b + \cdots = F_{(p_l)} \quad " \quad " \quad " \quad P_l.$$

From these equations the normal equations are derived in the usual way and their solution gives the most probable values of the constants of the function $f(x)$. Introducing these values in the observation equations one obtains the deviations of the calculated from the observed results and the sum of the squares of these deviations permits to state the accuracy obtained in the determination of the constants of the psychometric functions.

The method of calculation explained here is based on two

suppositions which must not be overlooked. The first supposition says that the nature of the dependence between the intensity of the comparison stimulus and the probabilities of the different judgments is known, whereas the second supposition specifies this assumption still further by admitting the existence of the function $F(p)$ with all its qualities. This assumption is not necessary, because one can solve the problem without it, but it is extremely convenient and facilitates the calculation considerably. A further argument in favor of this restriction of the problem is the fact that the existence of the function $F(p)$ was tacitly assumed in all previous investigations. No objection can be raised against this supposition, unless one proposes to solve the general problem, an undertaking which is more laborious than difficult.

The question as to the justification of the first hypothesis offers an entirely different aspect. A hypothesis on the psychometric functions is valid only when it expresses the actual dependence of the probabilities of the different judgments on the intensity of the comparison stimulus. The nature of this dependence is not known and cannot possibly be deduced by any considerations *a priori*. It is quite obvious that a function cannot be the object of any immediate experience, but that it must be found by regularities in the results of observation. No such knowledge is at hand at present, so that one hypothesis about the psychometric functions is just as arbitrary as any other. This is the peculiar difficulty of this problem, that one is forced either to make a hypothesis which is not more justified than any other or that one must treat the results by Lagrange's (or some analogous) formula of which one knows positively that it cannot be correct and which takes in errors which are absolutely unknown as to their size and sign.

It seems to be best to test the experimental material at hand by different hypotheses and not to regard any one of them as final, no matter what its success may have been. What we really need is a standard by which we can judge the different hypotheses, because if we have one, we can discard certain mathematical expressions as unsuitable to represent the psychometric functions. The treatment of the same data by different hypotheses furnishes

valuable indications. If the constants of the psychometric functions are known we can calculate the probabilities of the judgments according to the different hypotheses and compare them with the results of the observations. The sum of the squares of these deviations is the criterion of the value of a hypothesis. It is not possible to arrive at a final conclusion in this way, because an infinity of hypotheses would have to be gone through, but it may be decided which one of a certain set of mathematical expressions is best suited to represent the psychometric functions. The number of functions which for practical purposes come under consideration as hypotheses on the dependence of the probabilities of the judgments on the intensities of the comparison stimulus is naturally rather limited. The process of calculating the constants of the psychometric functions is rather laborious and it becomes the more so the more complicated the hypotheses are. For this reason one will not be inclined to take up the study of very complicated functions, unless there are strong arguments in favor of them. It may be expected that among these functions there is one which is more suitable as a hypothesis about the psychometric functions than all the others. This hope is as well compatible with the view that the form of the psychometric functions is the same for all subjects, as with the more conservative view that these functions differ for different individuals and perhaps even for the same individual at different times.

We may regard a hypothesis on the psychometric functions merely as a mathematical expression which fits one set of experimental data well, and another, perhaps, less satisfactorily, but we consider it from the start as extremely unlikely that an expression can be found which fits all data equally well. One may support this view by referring to the individual differences between the different subjects, which seem to exclude any such regularity. If, contrary to expectation, such an expression is found we will not have to change our views materially and benefit by this discovery for the facilitation of future work.

The problem which confronts us in the study of the psychometric functions is similar to the problem of determining the probability of dying as a function of age. A mathematical ex-

pression which gives this dependence is called the biometric function. These functions are *a priori* just as unknown as the psychometric functions are and the same difficulties are encountered in their *a posteriori* determination, but experience shows that there exists a formula, the so-called formula of Gomperz-Makeham, which has done better service than any other formula tried for this purpose. One therefore expects a similarly satisfactory result for the future and one naturally turns to this formula if new material is to be treated. The modern view about the biometric functions is similar to the one which we gave for the psychometric functions, namely that it is impossible to find an expression which fits all data equally well, a view which is not only supported by past experience, but which also may be backed up by the argument that the conditions under which men live are so different that the existence of any such regularity seems very unlikely. Experience must show whether there is less difference in the psychological make-up of people, but meanwhile we may undertake to find out how different hypotheses on the psychometric functions work out in their application to the results of observation.

We will consider here two hypotheses on the psychometric functions, in which the probabilities of the equality cases are expressed in terms of those of the extreme judgments. The psychometric function of the smaller judgments may be represented by the expression

$$f(x) = \frac{1}{\sqrt{\pi}} \int_{h_1(x-a_1)}^{\infty} e^{-t^2} dt$$

This expression is admissible as a hypothesis on the psychometric function of the smaller judgments, because it decreases with increasing intensity of the comparison stimulus and approaches the limit 1 for $x = -\infty$ and the limit 0 for $x = \infty$. One easily sees that the expression

$$h(x) = \frac{1}{\sqrt{\pi}} \int_{-\infty}^{h_2(x-a_2)} e^{-t^2} dt$$

is admissible as a hypothesis on the psychometric function of of the greater judgments. The probabilities of the equality

judgments are given by

$$g(x) = \frac{1}{\sqrt{\pi}} \int_{h_1(x-a_1)}^{h_2(x-a_2)} e^{-t^2} dt.$$

The three functions $f(x)$, $g(x)$, $h(x)$ contain only the constants a_1, a_2, h_1 and h_2 and they are fully determined by them. These constants must be determined in such a way as to fit the data from which they are deduced as well as possible.

This hypothesis may be called the $\Phi(\gamma)$-hypothesis. It is remarkable and well known for the fact that G. E. Mueller uses it in his method of constant stimuli. Mueller starts from the notion of a threshold which is subjected to chance variations, the frequency of which is a function of their size. The mathematical expression for the probabilities of the variations as depending on their size is called their law of distribution. Mueller and his followers assume the exponential law, which frequently but not very appropriately is called the Gaussian law, to hold good for the distribution of the threshold. The ordinate of the maximum of this function is an axis of symmetry, which gave rise to the well-known discussion whether it was admissible to make the assumption that the variations of the threshold follow a symmetrical law of distribution. This objection was strengthened by the fact that all empirical distributions studied until now show an essential asymmetry which is sometimes small but sometimes very considerable indeed. It is not possible to say that the discussion of this problem was very fruitful of important results.

The question as to the symmetry of the law of distribution has the following meaning for the psychometric functions. $f(x)$ assumes the value ½ for $x = a$. Keeping in mind that the probability integral from zero to any positive limit is equal to that from zero to the same negative limit, we see that $f(a - x)$ and $f(a + x)$ are symmetric to the value 1/2. The curve representing $f(x)$ may be divided into two parts each one of which goes over into the other by being mirrored at the lines $y = ½$ and $x = a$, the order of this process being indifferent. It is quite obvious that this implies a very special hypothesis on the psychometric functions, but any other hypothesis has to be equally specific, and if one is thoroughly imbued with the con-

viction that nothing is definitely decided by a provisory acceptance of a hypothesis, one will not attribute too much importance to this question.

It is necessary for the practical application of this method to have a table of the values of this function. One either may construct a table similar to the well-known fundamental table for the method of right and wrong cases or one may use a table of the probability integral. Fechner's table is very convenient for working out sets of 25, 50 or 100 experiments, but in all the other cases it is more convenient to use a table of the proba-

TABLE III.

p	γ	Difference.	p	γ	Difference.
0.50	0.0000	177	0.76	0.4994	230
0.51	0.0177	178	0.77	0.5224	236
0.52	0.0355	177	0.78	0.5460	242
0.53	0.0532	178	0.79	0.5702	249
0.54	0.0710	178	0.80	0.5951	257
0.55	0.0888*	179	0.81	0.6208	265
0.56	0.1067*	180	0.82	0.6473	274
0.57	0.1247	180	0.83	0.6747	284
0.58	0.1427*	182	0.84	0.7031*	298
0.59	0.1609	183	0.85	0.7329	310
0.60	0.1792*	183	0.86	0.7639	326
0.61	0.1975	185	0.87	0.7965	347
0.62	0.2160	186	0.88	0.8308	365
0.63	0.2346*	189	0.89	0.8673	389
0.64	0.2535	190	0.90	0.9062	418
0.65	0.2725	191	0.91	0.9480*	455
0.66	0.2916*	195	0.92	0.9935*	500
0.67	0.3111	196	0.93	1.0435*	558
0.68	0.3307	199	0.94	1.0993*	637
0.69	0.3506	202	0.95	1.1630*	750
0.70	0.3708	205	0.96	1.2380*	920
0.71	0.3913	208	0.97	1.3300*	1220
0.72	0.4121	212	0.98	1.4520*	1930
0.73	0.4333	216	0.99	1.6450	
0.74	0.4549	220	1.00	∞	
0.75	0.4769	225			

bility integral. The tables of Kaempfe [1] and of Bruns [2] are easily accessible to psychologists and they have the great advantage that the interval of the table is very small. The values of the psychometric function calculated from the table of Bruns do not always coincide with the data of the table of Fechner, which is reprinted in most of the treatises on psychophysical measure-

[1] B. Kaempfe, *Psychologische Studien*, 1893, Vol. 9.

[2] H. Bruns, *Wahrscheinlichkeitsrechnung und Kollektivmasslehre*, 1906.

ment methods, and it seems advisable to give here a table of the values calculated from the table of Bruns. The arrangement of this table is identical with that of the fundamental table for the method of right and wrong cases; values marked by an asterisk (*) differ from the corresponding values in Fechner's table. Owing to the symmetry of the function it is sufficient to give the values between 0.50 and 1.00.

The next step consists in finding the formula for the weights of the observation equations. Following the line of argumentation given above we find for the weight of the observation with a comparison stimulus

$$P = \frac{1}{2\pi} \cdot \frac{se^{-2\gamma^2}}{p(1-p)}$$

where s is the number of experiments made. This formula corresponds to the expression which G. E. Mueller gave for the weight of an observation equation, but it differs from it in such a way as to give results which are greater than the corresponding values of Mueller. An analysis of this formula shows (1) that the function representing the weight of the observation equations has a maximum at $p = \frac{1}{2}$ and that the ordinate of this maximum is an axis of symmetry of the function, and (2) that the function assumes the value zero for $p = 1$ and for $p = 0$, showing that observations which gave the frequency 0 or 1 for one of the extreme judgments are without influence on the determination of the constants of the psychometric function of this judgment, from which it follows that these results simply may be omitted. It is best to use comparison stimuli which give probabilities not differing much from $\frac{1}{2}$, because these observations come down with the greatest weight. It is convenient to have a table of these weights, which owing to the symmetry of the function representing them need cover only one of the intervals from 0 to 0.5 or from 0.5 to 1. Table IV. contains these values for the interval from 0.5 to 1; the arrangement and the use of this table is identical with that of Mueller's table.

The process of setting up and solving the normal equations derived from the observation equations is well known and need

TABLE IV.

WEIGHTS OF THE OBSERVATION EQUATIONS ACCORDING TO THE $\Phi(\gamma)$-HYPOTHESIS.

p	P	Difference.	p	P	Difference.
0.50	1.000	0	0.76	0.832	14
0.51	1.000	1	0.77	0.818	15
0.52	0.999	1	0.78	0.803	16
0.53	0.998	2	0.79	0.787	17
0.54	0.996	1	0.80	0.770	18
0.55	0.995	3	0.81	0.752	19
0.56	0.992	3	0.82	0.733	20
0.57	0.989	4	0.83	0.713	19
0.58	0.985	4	0.84	0.694	24
0.59	0.981	4	0.85	0.670	24
0.60	0.977	5	0.86	0.646	25
0.61	0.972	5	0.87	0.621	26
0.62	0.967	7	0.88	0.595	28
0.63	0.960	6	0.89	0.567	29
0.64	0.954	7	0.90	0.538	32
0.65	0.947	7	0.91	0.506	34
0.66	0.940	8	0.92	0.472	37
0.67	0.932	9	0.93	0.435	39
0.68	0.923	9	0.94	0.396	44
0.69	0.914	10	0.95	0.352	48
0.70	0.904	10	0.96	0.304	55
0.71	0.894	11	0.97	0.249	62
0.72	0.883	12	0.98	0.187	78
0.73	0.871	12	0.99	0.112	112
0.74	0.859	13	1.00	0.000	
0.75	0.846	14			

not be described here, but a few remarks may prove useful. The opinion is very widespread that this process is difficult or at least very laborious. This is not the case if the computations are arranged properly. With all the necessary checks the calculations ought not to take more than two hours even if one has only little practice in this work. A scheme of calculation which has stood the test of practical application is given in the *Archiv f. d. ges. Psychologie*, Vol. 16.

We give here the results of the computation for the seven subjects in our experiments on lifted weights. The first two columns of Tables V. and VI. give the constants of the psychometric functions and the columns under the headings S_1 and S_2 give the lower and upper limit of the interval of uncertainty. These values may be compared with the results of calculation and observation by the method of just perceptible differences and by Lagrange's formula of interpolation. The results of

TABLE V.

CONSTANTS OF THE PSYCHOMETRIC FUNCTION OF THE SMALLER JUDGMENTS BY THE $\Phi(\gamma)$-HYPOTHESIS. LOWER LIMIT OF THE INTERVAL OF UNCERTAINTY.

Subject.	h_1	c_1	S_1	Method of Just Perceptible Differences.		Interpolated by *Lagrange's* Formula.
				Calculated.	Observed.	
I.	0.125777	11.7398	93.34	93.49	93.30	93.26
II.	0.105252	10.0074	95.08	94.98	94.87	95.20
III.	0.138920	13.6010	97.91	97.88	97.85	98.65
IV.	0.123357	11.7730	95.44	95.56	95.39	95.24
V.	0.127380	12.0011	94.22	94.57	94.47	93.75
VI.	0.110215	10.5023	95.29	95.20	95.31	95.82
VII.	0.114453	10.9650	95.80	96.74	95.79	95.33

these different methods agree so well that no further discussion is needed.

It is a notorious fact that in former investigations the method of constant stimuli did not give the same results as the method of just perceptible differences. This was due to an imperfect understanding of the method of just perceptible differences and one may safely say that the difficulties of the method of constant stimuli were largely due to those of the method of just

TABLE VI.

CONSTANTS OF THE PSYCHOMETRIC FUNCTION OF THE GREATER JUDGMENTS BY THE $\Phi(\gamma)$-HYPOTHESIS. UPPER LIMIT OF THE INTERVAL OF UNCERTAINTY.

Subject.	h_2	c_2	S_2	Method of Just Perceptible Differences.		Interpolated by *Lagrange's* Formula.
				Calculated.	Observed.	
I.	0.136113	13.5676	99.68	99.60	99.45	100.95
II.	0.110945	11.0138	99.27	98.71	98.83	99.55
III.	0.145240	14.4346	99.39	99.58	99.28	100.32
IV.	0.117995	11.5917	98.24	98.24	98.08	98.26
V.	0.115708	11.2424	97.16	97.35	97.14	95.83
VI.	0.114995	11.5862	100.75	100.33	99.86	101.04
VII.	0.115465	11.6816	101.17	99.63	99.86	100.74

perceptible differences. As long as one did not see that the result of this latter method could be defined in terms of the probabilities of the different comparison stimuli, one could not possibly use the same material for a test of both methods, and

it is very difficult to obtain different sets of results under exactly the same conditions. This is particularly true with reference to experiments made by the method of just perceptible differences in its traditional form and those of the method of constant stimuli, because in the first case one has to present the stimuli in a certain order (ascending or descending), whereas they may be given in random order in the method of constant stimuli. For this reason it is not possible in the method of just perceptible differences to keep the subject in ignorance as to the direction in which the stimuli are varied. This circumstance causes a difference in the attitude of the subject which must influence the judgment, and for this reason it is not very likely that the experimental data obtained by the two methods are strictly comparable.

Differences in the conditions of the experiments may be detected in two ways, by introspection or by a difference in the objective results. We take the view that introspective evidence against a set of experiments makes it suspect, but the absence of any such objection does not put the value of the material beyond doubt. This is the view which is taken in the theory of observations where a set of observations must not be judged off-hand but only on the basis of a minute examination. Differences in the objective results of psychophysical experiments are differences in the values of the observed probabilities and we may call the conditions of two groups of experiments materially different if the judgments have not the same probabilities for the comparison of the same stimuli. Arrangements which are not identical, but which do not interfere with the values of the probabilities of the different judgments, are said to be only formally different. The method of just perceptible difference when looked at as a method of calculation is only formally different from the method of constant stimuli, because, as Tables V. and VI. show, both methods give the same results. The method of just perceptible differences in its traditional form, however, requires results of special kind which are materially different from those obtained and used in the method of constant stimuli. This accounts for the differences thought to exist between these two methods.

The problem of finding the relation between the different psychophysical methods has a well-defined meaning only if one refers to results obtained under conditions which are not materially different. The problem of psychophysics in general is to determine the influence of the conditions of the experiments on our judgment, those conditions being of primary importance which depend on the state of the subject. Various methods have been devised for the purpose of this analysis, the formal character of which has to be perfectly understood before a conclusion can be drawn as to the material difference or identity of the conditions in the different sets of observations. We may express this idea by saying that the purpose of the psychophysical measurement methods is an analysis of the material conditions which determine our judgment on the comparison of stimuli, but for this purpose an understanding of the formal character of these methods is needed.[1]

When the constants of the psychometric functions are known one may calculate the probabilities of the different judgments for all intensities of the comparison stimulus. The results of this calculation are given in Table VII. The numbers under the heading 'observed values' give the difference between the

[1] The distinction between formal and material conditions of an experiment was not favorably criticized in the discussion following the reading of the paper, perhaps because it was not as clearly presented as it might have been. This distinction, however, is absolutely indispensable for a proper understanding of the psychophysical methods. Experience shows that the results of the method of just perceptible differences coincide with those of the method of constant stimuli if the same material is used for this test, and that they are different if different materials are used. There is obviously no reason why the results should agree in one case and not agree in the other, unless there are some similarities between the two methods, which are counteracted by the differences of the conditions under which the material was obtained. In the monograph on 'The Application of Statistical Methods to the Problems of Psychophysics' the emphasis was laid on the formal identity of the method of just perceptible differences with the error methods, because it was a new observation that both methods give the same result if they are tested on the same material. Mr. G. Geiger (*Zeitschrift f. Psychologie*, 1910, Vol. 54, pp. 540–542) in his review of this book and Miss H. D. Cook in her treatise on 'Die taktile Schätzung von ausgefülten und leeren Strecken,' *Archiv f. d. ges. Psychologie*, 1910, Vol. 16, p. 455, justly point out that the differences between the experiments made according to these two methods are not sufficiently dwelt upon. This remark is perfectly to the point and shows the importance of making the distinction between formal and material conditions of an experiment.

TABLE VII.

VALUES OF THE PSYCHOMETRIC FUNCTIONS.
COMPARE CHART 2.

Comparison Stimulus.	Smaller.		Equal.		Greater.	
	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.
80	0.9876		0.0011		0.0113	
82	0.9742		0.0030		0.0228	
84	0.9504	—0.0171	0.0075	+0.0147	0.0421	+0.0023
86	0.9117		0.0172		0.0711	
88	0.8540	+0.0082	0.0357	—0.0113	0.1103	+0.0030
90	0.7752		0.0682		0.1566	
92	0.6767	+0.0233	0.1200	—0.0089	0.2033	—0.0144
94	0.5639		0.1946		0.2415	
96	0.4456	+0.0033	0.2920	+0.0013	0.2624	—0.0046
98	0.3319		0.4076		0.2605	
100	0.2320	—0.0009	0.5319	—0.0030	0.2361	+0.0039
102	0.1515		0.6532		0.1953	
104	0.0922	+0.0034	0.7605	+0.0551	0.1473	—0.0584
106	0.0520		0.8465		0.1015	
108	0.0272	—0.0116	0.9091	—0.0047	0.0637	+0.0163
110	0.0132		0.9504		0.0364	
112	0.0059		0.9752		0.0189	

calculated and the observed values of the probabilities, the sign being determined so as to make the arithmetic sum of the terms equal to the observed value. The differences between the calculated and the observed values are very small and some of them are negative and some positive, both signs being distributed quite irregularly throughout the table. The data of this table may be represented graphically, as it is shown in Chart 2. The construction of the curves in this chart is the same as that of Chart 1, so that it need not be explained again. It is seen at a glance that the course of the curves is regular throughout the table and that the ascent and descent of the curves representing the psychometric functions of the extreme judgments is not interrupted by any secondary elevations.

We now turn to the study of another hypothesis on the psychometric functions. One sees immediately that an expression of the form

$$f(x) = \frac{1}{2} - \frac{1}{\pi} \arctan (ax + b)$$

may represent the psychometric function of the smaller judgments, because it approaches the values zero and one asymp-

totically and decreases for increasing values of the comparison stimulus. For similar reasons is it admissible to suppose that such a function may represent the psychometric function of the greater judgments, if the sign and the constants of the term arctan are determined appropriately. This hypothesis leads to a computation similar to that of the first hypothesis and the necessary formulæ may be easily found by the considerations given above,[1] but for our present purpose only the question is of importance whether this hypothesis agrees better with experience than the $\Phi(\gamma)$-hypothesis.

Table VIII.

Sums of the Squares of the Deviations of the Calculated from the Observed Values.

Subject.	Arctan-Hypothesis.			$\Phi(\gamma)$-Hypothesis.		
	Smaller.	Greater.	Equal.	Smaller.	Greater.	Equal.
I.	0.019879	0.016931	0.024358	0.001738	0.022826	0.019738
II.	0.010243	0.010667	0.011302	0.001060	0.003492	0.003934
III.	0.019196	0.016839	0.014051	0.008008	0.011209	0.002759
IV.	0.013797	0.020357	0.004427	0.003422	0.004552	0.002023
V.	0.012195	0.036444	0.015876	0.014093	0.011771	0.000743
VI.	0.016717	0.008770	0.017292	0.002878	0.003991	0.003875
VII.	0.022457	0.014181	0.025161	0.002707	0.009569	0.007986
Average	0.016355	0.017741	0.016067	0.004844	0.009630	0.005865

For this purpose it is necessary to calculate the values of the probabilities of the different judgments according to both hypotheses and form the differences between the observed and the calculated values. These deviations are squared and their sums formed. These results are given in Table VIII., which contains the sums of the squares of the deviations for each one of the seven subjects in our experiments on lifted weights. A mere glance at this table shows that the judgment as to the value of the two hypotheses in question cannot be doubtful for a minute. With the only exception of the psychometric function of the smaller judgments for subject V., all the sums of the squares of the deviations calculated by the $\Phi(\gamma)$-hypothesis are smaller than those calculated by the arctan-hypothesis. We,

[1] All the details of the calculation are given in the treatise on 'Die psychophysischen Massmethoden als Grundlagen empirischer Messungen,' *Archiv f. d. ges. Psychologie*, 1909, Vol. 16.

therefore, must say that the first hypothesis fits our results better than the second.

A closer examination of the data of Table VIII. shows that the agreement between the results of calculation and of observation is different not only for different subjects, but also for the different judgments. The averages of these sums for our seven subjects are 0.014767, 0.002829, 0.007325, 0.003332, 0.008869, 0.003581 and 0.006754. It is perhaps worth while noticing that subject II., who had by far the greatest practice in psychological experiments, has the smallest average, whereas subjects III. and V., who were the least reliable, have large averages. From this one might conclude that the psychometric functions approach the $\Phi(\gamma)$-type with increasing practice of the subject, but this conclusion does not agree with the fact that subjects I. and VII. have a large average, although their reliability manifests itself by a small coefficient of divergence. The averages of the sums of the squares of the deviations for the three judgments are given at the bottom of each column in Table VIII. The agreement between theory and observation is best for the smaller judgment, the second place being taken by the equality judgments, the third by the greater judgments. This result agrees very well with the standpoint we have taken before, that the psychometric functions may differ from individual to individual, and that the nature of the dependence of the probabilities on the intensity of the comparison stimulus may be different for the different judgments.

A MARKED CASE OF MIMETIC IDEATION.[1]

BY STEPHEN S. COLVIN,

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In the present discussion I wish to emphasize a point of view which I have set forth elsewhere[2] in regard to the nature of the mental image, namely that it is not always, perhaps not generally, due to centrally aroused processes alone, but that in part, at least, our imaginal experiences have mixed with them peripheral factors in such a way that it is impossible to separate these experiences out. This is particularly true of motor imagery. Indeed it may be doubted whether we ever experience a motor image in the sense of a purely cortical process. What is called a motor image may be in reality a motor sensation. Because of this difficulty of separating the centrally aroused processes from those peripherally aroused (especially if these latter are weak and indefinite), and because of the uncertainty attached to the term motor imagery, I have chosen the term motor ideation in the present paper, meaning to signify by this term those kinæsthetic experiences (whether central or peripheral in their origin) which we employ at times in our thinking and which constitute the mind-stuff of certain of our ideational processes.

It is common to divide ideational types into two main classes, namely, object types (*Sachvorstellungstypen*) and word types (*Wortvorstellungstypen*). Meumann, for example, enumerates under the former, beside the concrete visual, acoustic and tactile-motor types,— gustatory, olfactory and emotional types; further, motor types in which the ideational processes are in terms of imitative movements. Under the word types he enumerates the verbal-visual, the verbal-acoustic, and the verbal tactile-motor types.

[1] This paper was read before the American Psychological Association, at Cambridge, Mass., December 30, 1909.

[2] PSYCHOL. REV., 1908, XV., 158-168.

The writer of the present paper in introspecting concerning his own ideational processes, finds himself continually employing a kind of ideation that does not seem to be clearly brought out in this classification, nor to be emphasized to any extent in the literature. He is almost exclusively of a pure motor type in his ordinary thinking, although he does have the ability to revive at will concrete visual imagery of pronounced richness and variety; further, he has marked visual dream hallucinations and has at times experienced such hallucinations in his waking moments. Under certain circumstances he can mentally picture colors that give the impression of being more highly saturated than any he has ever seen in the spectrum. Notwithstanding this ability to recall sensory objects in such concrete ideational forms, in his ordinary thinking he only on rare occasions employs verbal-visual ideation. He does not see the printed or written word, and cannot readily tell from the visual appearance of a word whether it is properly spelled or not. He has no auditory ideation whatsoever, whether concrete or verbal, and so it happens that practically all of his thought processes are of the motor type.

With insignificant differences except for one important addition, his motor ideas are of the same character as those reported by Stricker[1] and by Dodge.[2] In his concrete motor ideation he finds like Stricker that when he recalls movements of his own body or of other persons or animals, he gets suggestions of muscular contraction in those parts of the body concerned in the movements. In following objects in motion or in recalling their movements, he, like Stricker, is conscious of a sensation or image of strain of the eye muscles. Beyond this, when he attempts to image certain objects that are at rest, as for example a circle, he finds that a part, perhaps all, of the visual idea is really kinæsthetic. He gets the clear impression in trying to recall the circumference of the circle, of eye movement in the orientation of the outlines of the figure, and of eye fixation when he turns his attention to the center. For this reason

[1] *Studien über die Sprachvorstellungen*, 1880, und *Studien über Bewegungsvorstellungen*, 1882.

[2] *Die motorischen Wortvorstellungen*, Halle, 1896.

he finds difficulty in recalling both circumference and center at the same time, and when he succeeds in doing this he is distinctly conscious of changing his type of ideation from motor to visual. In general he finds that his recall of the outlines of visible objects is distinctly a kinæsthetic and not a retinal affair.

When he introspects concerning his verbal ideation he finds that he is largely speech-motor in his thinking. He notices in general the same phenomena that are reported by Stricker and by Dodge in their respective accounts of their internal speech. There are the same rather definitely localized sensations from the lips, the tongue and the larynx as set forth by Stricker and amplified in detail by Dodge. In addition there is often the sensation of a rather diffused internal pressure at the base of the tongue, streaming upward toward the soft palate and with the distinct feeling of tension within the ear itself, accompanied, however, by not the slightest suggestion of a genuine auditory image. These sensations of pressure and tension are particularly marked when the writer is engaged in the silent reading of a foreign language, and have been observed at their maximum in learning nonsense syllables that are difficult to pronounce.

While the various ideational processes, thus briefly outlined, in a few minor particulars differ from what has already been reported concerning the extreme motor type, they do not, in the main, offer any facts strikingly new or significant. As has already been said, however, the writer finds in one particular, as yet not touched on, that his motor ideation is somewhat novel, and perhaps significant, in as much as he finds over and above what has already been definitely recorded in the literature a kind of motor thinking which may be termed, for lack of a better name, dramatic or mimetic. This dramatic or mimetic ideation is both concrete and symbolic. In its concrete form it may very well fall under the classification of Meumann, previously referred to under imitative movements in connection with concrete thought processes. If the writer recalls vividly such a situation as he sometimes has experienced on a still evening in the summer on the shores of Narragansett Bay, when the sandflies have made life decidedly disagreeable, he is apt to reinstate

this situation in terms of a rapid movement (either actually executed or merely imagined) of his hand past his ear and face. In this case there is an actual revival of a situation in motor terms which is just as concrete and definite as if he had recalled in tactile terms this discomfort which the flies had caused him. There are many experiences of this sort where the image or the sensation of the movement becomes the vital aspect of the situation.

The novel aspect of this type of ideation, however, is not to be found in the reinstatement in a concrete way of these experiences, but in the symbolic use of this mimetic ideation, where the movements, either executed or merely recalled, are not definite reactions to a set of definite objective conditions, but rather become signs of such an objective reality, in much the same manner as words seen, heard or spoken are symbolic of the realities for which they stand. In this case the writer finds himself using not indeed an inner speech, but an inner sign language which carries for him the meaning often of abstract and colorless modes of thought. Just as words have become conventionalized so that their significance as actual processes of adjustment have in most instances been lost, so this mimetic ideation often does not apparently possess any marked significance as representing definite adjustments to a concrete situation, although at times it is expressive in a way in which words often are not. Apparently these mimetic ideas represent in part instinctive movements, and in part certain conventionalized gestures which have been acquired in individual experience, and which at one time may have had a concrete significance in adjustment, but which now have passed almost entirely from the concrete to the symbolic.

A few illustrations may make clear what the general nature of this symbolic mimetic ideation is. When the writer attempts to call to mind a series of words, a phrase, a paragraph, or even a collection of nonsense syllables, his first experience is not generally the recall of the words as such, but rather the emergence into consciousness of the background or setting of these words in terms of their general 'drift' or meaning. There seems to be a certain sort of rhythmical sequence in

which these words appear; what would be a spatial arrangement for one who thought in visual terms; not the spatial arrangement, however, of the words as they would appear on a printed page, but a spatial arrangement in terms of their meaning in which the transitional thought processes are represented by curved or zigzag movements; in which often there appears a sort of a plot with a distinct rise to a climax and perhaps a falling off at the end. This way of thinking comes out most clearly when the writer attempts to learn a series of nonsense syllables. If left to his own devices he finds himself invariably attempting to arrange these, even when he studies them in successive presentation, in a sort of a sequence of movements which seem to be essential to their subsequent recall. Unless he can recall in this way the general total movement he is often at a complete loss to revive the words that he has previously learned. When, however, he is able to reinstate the original background, then the words themselves come up readily and with comparatively little loss, even after a period of one or two days. On the other hand, if he attempts to learn a word series by mere *Einprägung* he finds such learning extremely slow and difficult, and he finds a corresponding difficulty in subsequent reinstatement.

The actual mind-stuff of this ideation he finds in sensations or images which represent gestures such as pointing, raising the index finger, curving the hand, and in more general kinæsthetic 'sets' which may involve the entire musculature. Such a sentence as this comes to the mind, 'Infinity broods over all things.' Immediately with the words themselves come into consciousness the speech-motor processes and further a general background of kinæsthetic symbolism. The kinæsthetic symbol for *infinity* is found in the tendency to prolong the word, this prolongation being accompanied by the distinct impression of projecting it from the mouth and then following this projected word by definite bodily movements. There is an image or sensation of a forcible and continued ejection by the speech-motor apparatus and of a bending forward and tension of the entire body, setting itself as if for flight. There is no visual symbol here, as for example, of extended space, or the

limitless vault of the heavens on a starlit night. The whole comes in motor adjustments. The word *broods* brings an entirely different suggestion. Here the ideation centers in a distinct picture (kinæsthetic not visual) of outstretched hands, and body bending forward and downward. *All* is symbolized by a sensation or image of roundness in the oval cavity, and by an extensive gesture (not actually executed, but merely represented) of an inclusive movement with both hands sweeping around and joining in front of the body. The symbol for *things* is the mental representation of a direct and sudden gesture with hand extended and index finger pointing out and downward.

It may be noticed from this description that much of this mimetic thinking is in the stage of what would correspond in the development of word-language to the period of ideographs in which the word-symbol suggests the concrete situation for which it stands. This is true particularly of the ideation in connection with the word *broods*. On the other hand *infinity* and *things* are represented by imaginal elements in which the concrete has been almost entirely replaced by the symbolic.

One further example may serve as an additional illustration of how this kinæsthetic ideation is employed. Recently two members of my beginning class in psychology reported to me that when they attempted to recall the expression *words seen*, they got beside the visual image of the words themselves, a picture of a curved line which for them symbolized the inflection of the voice in pronouncing these words. Here evidently was a case of visual symbolic imagery that formed a background to the words themselves. In the writer's case the whole would come in a purely kinæsthetic form. The words would be recalled solely in speech-motor terms, and the inflection of the voice in purely kinæsthetic ideation, either localized in the speech-organs themselves or represented by an imagined gesture (not seen but recalled in terms of motor adjustments) of the index finger describing the curve.

The writer's attention was first clearly called to his own excessive use of kinæsthetic ideation, when he attempted to set forth in an article in Woodbridge's journal [1] and later in a series

[1] *Journal of Philosophy, Psychology and Scientific Methods*, II., 229, 1905.

of discussions in the *Philosophical Review*[1] his standpoint concerning the noëtic intention. This involved analyzing out the mindstuff which formed the background of his experience in objectifying, in intending, in pointing. He then concluded that this intention had at its basis 'experienced sensations of muscular adjustment.'

Later in introspecting on his normal method of learning and recalling nonsense syllables, he became convinced that his kinæsthetic ideation, beyond and in excess of his speech-motor thinking, is the essential element in meaning for him. More and more he has become impressed with the fact that mimetic ideation is to a large extent the actual texture of his thought processes, and that while without careful analysis he might come to the conclusion which various psychologists have reached, that thought can flow on without an imaginal background, he is forced on making such an analysis to the conclusion that, although in his own thinking there is a marked lack of concrete or verbal ideation, he has a rich content of an imaginal nature in his mimetic imagery.

This belief was still further confirmed by a series of lectures delivered by Professor Titchener at the University of Illinois in March, 1909, and just issuing from the press. The subject of these lectures was the 'Experimental Psychology of the Thought Processes,' and the conclusion reached by the lecturer was in brief that it would not be surprising to discover "for minds of a certain constitution all conscious meaning is carried either by total kinæsthetic attitude or by words." The writer is convinced that his experience is a case in point.

While this type of ideation is probably not usual, I have reason to believe that it may be more common than may at first be supposed.[2] In a conversation with President G. Stanley Hall recently, I was informed by him that he possessed this type of thinking to a marked degree. He was kind enough to read to me an unpublished note describing his method of learning, when a boy, certain meaningless words and his method of re-

[1] 'The Intention of the Noëtic Psychosis,' XV., 307–311, 515–517, 1906.

[2] In the discussion which followed the presentation of this paper about one third of those present at the session reported that they possessed this type of ideation to a greater or less extent.

membering musical compositions. In his case the mimetic ideation is largely in imaged or partially executed dances and in other rhythmical movements that, however, are evidently in part concrete and in part symbolic adjustments to the actual situation.

I have also recently found still a third person who seems to be of this same general type; a person with marked motor ideation, with ineffectual visual ideation, and without auditory ideation; may it not be that there are many such cases, and is it not further likely that persons of a predominatingly visual or auditory type have a measure of this mimetic ideation: and may it not be that such persons are wrong in believing that their thought at times has no sensory or imaginal background, and that a more careful analysis would lead them to the discovery of just this thing which in a person of a pronounced motor type comes more noticeably to the foreground?

In conclusion, I wish to urge that the existence of this type of ideation is what we might naturally expect. The current biological theories in regard to consciousness, particularly the instrumentalism of Dewey, have made us familiar with the thought that the meaning of a situation is after all an attitude and that this attitude must be in the last analysis a motor affair. Judd has well expressed this in a recent article.[1] Here he says, "unity of percepts and unity of ideas are . . . phases which describe an aspect of consciousness dependent on motor tendencies."

It is but a step further to the conclusion that this general dependence of experience for its significance or motor adjustment has left a deposit of mind-stuff that symbolically represents concrete situations, not actually present, but thus ideally represented. This mind-stuff may in many instances have lost entirely its original significance and, as Baldwin has expressed it[2] in discussing motor attitudes expressive of emotions, may have resulted in apparent confusion 'due to the grinding, erosion, rivalry of development processes among themselves.' Thus those attitudes which once had a definite significance as attitudes

[1] *Journal of Philosophy, Psychology and Scientific Methods*, 'Motor Processes and Consciousness,' Feb. 18, 1909.

[2] *Mental Development*, third edition, p. 248, 1906.

having lost their specific meaning, are capable of taking on a symbolic character, thus functioning for meaning without themselves being as such concretely significant. In other words, the situation is no longer reinstated in terms of an actual, concrete adjustment, but rather in terms of a kinæsthetic symbol of that adjustment.

FROM THE UNIVERSITY OF CALIFORNIA PSYCHOLOGICAL LABORATORY.

XI. Experiments on the Reproduction of Distance as Influenced by Suggestions of Ability and Inability.

BY GRACE MILDRED JONES, M.L.

Up to the present time the several experiments to determine the effect of suggestion have been made with some important differences in the method of investigation. In the earlier experiments the observer was given no instructions to resist any influence by suggestion; as, for example, in Small's [1] test, where a visual illusion was employed and the children given no warning of such; so in Binet's [2] experiment where the subjects were misinformed as to the true length of the lines in their relation to one another. The experiments of Pearce,[3] and of Smith and Sowton [4] were made under practically these same conditions.

With Brand [5] a radically new method was adopted. The subjects were aware of the purpose of the experiment and while the content of the suggestion was to be given place in the mind the observers were warned against any voluntary response to it. Furthermore, the reactions depended not alone upon visual perception but primarily upon the power to reproduce. Bell's [6] experiment in these essential conditions was identical with Brand's. It differed chiefly in this: that a visual type of suggestion was used in addition to the vocal.

In the present investigation, the method in general remained

[1] Small, 'The Suggestibility of Children,' *Pedagog. Sem.*, 1896, IV., pp. 176–220.

[2] Binet, La suggestibilité, *Annee Psych.* V., pp. 82–152.

[3] H. J. Pearce, 'Normal Motor Suggestibility,' PSYCH. REV., 1902, IX., pp. 329–356.

[4] W. J. Smith and S. C. M. Sowton, 'Observations on Spatial Contrast and Confluence in Visual Perception,' *Brit. J. Psych.*, 1907, II., pp. 196–219.

[5] J. E. Brand, 'The Effect of Verbal Suggestion on the Estimation of Linear Magnitudes,' PSYCH. REV., 1905, XII., pp. 41–49.

[6] J. C. Bell, 'The Effect of Suggestion upon the Reproduction of Triangles and Point Distances,' *American Journal of Psychology*, 1908, XIX., pp. 504–518.

the same as with Brand. The nature of the suggestions was changed and instead of being given in the form of a command they conveyed ideas of ability and inability. The chief difference lay in the fact that three types of suggestion were used, the vocal, the visual, and the 'auto,'[1] and that a special investigation was made of the relative effect of these various types.

Method of Experiment.

The apparatus employed in the present experiment was almost identical with that used by Brand. At a distance of 80 centimeters from the subject two white pegs were set up showing through a narrow slit in a black screen; below this slit was another, somewhat wider, in which were exposed the visual suggestions. It was so arranged that the slits might be covered and uncovered conveniently to meet the needs of the experiment. At a distance of 40 centimeters from the observer was another black screen low enough so that the subject was able to see the horizontal slits in the farther screen where the pegs fixing the standard distance were exposed. On the nearer screen was a ledge where the subject was to adjust corresponding pegs in making his 'reproduction.' Both screens were built upon a table at a convenient level; the background was black and the room but dimly lighted. The pegs themselves were made clearly visible by screened lights.

The observer responded to six varieties of suggestion and, to one signal where no suggestion was offered. Three types of suggestion were used — the visual, made by means of the printed mottoes "You are now able" and "You are now unable"; the vocal, made by the experimenter to the observer in the same words; and the 'auto,' made by the subject to himself in the words, "I am now able"; "I am now unable." The suggestions were given in irregular order; after each an interval of a second and a half was allowed before the exposure of the pegs, to give the subject time to concentrate his attention on the idea. The pegs were then exposed for a second and a half, the slide was replaced so as to conceal them, and the observer immediately

[1] 'Auto' is the name given to the type in which the observer responded to his own suggestion of ability or inability.

placed his pegs upon the ledge and adjusted them in accordance with his estimate of the distance between the exposed pegs. This distance, or space interval, was kept constant throughout the experiment; but of this the subjects were unaware, as their assertions prove. Each of the subjects took it for granted, when the absolute position of the pegs was changed, which occurred after every sixth judgment, that the space interval was changed also.

The experiment covered a period of five months, from October, 1908, to March, 1909; during this time the subjects were experimented upon at regular intervals and were required to give approximately the same number of judgments at each sitting. Three observers, experienced in psychological methods, were engaged. Five hundred twenty-five estimates were made by each of the three subjects; that is, seventy-five estimates for each variety of suggestion and seventy-five with no suggestion. The subject was instructed to allow each suggestion a place in his mind, to hold the idea it offered, but not to allow it consciously or intentionally to affect his estimate. Toward the close of the experiment each observer reported that he felt certain he was not being influenced in the least degree by any of the types of suggestion. According to their statements no feelings of ability or inability were aroused within them, nor was the experimenter able to observe any outward effect, such as hesitation or the like.

Results.

In that portion of Table I. which presents the averages for the combined seventy-five estimates the results are seen to be uniform for all the three subjects in these respects, that in all twenty-four 'groups'[1] where the affirmative of any type is compared with the negative of the same type, both the constant

[1] 'Group' is used here and throughout the account to mean a pair of averages, namely the affirmative and negative of any single type of suggestion or of the combined types — and this, either as regards average reproduction (or the constant error) or variability. For example, in Table I., in the first series of twenty-five judgments, 30.88 and 30.34 constitute a 'group' for subject *X*; 1.52 and 1.58 another group for the same subject. Thus for each series of twenty-five judgments there are eight groups for each subject, making twenty-four for the three subjects for that series.

error[1] and the variability[2] are appreciably less with the affirmative suggestions than with the corresponding negative suggestions. And again, in the averages obtained from judgments made with no suggestion there is always less constant error and usually less variability than in any of the averages of estimates with suggestion.

TABLE I.

THE AVERAGE REPRODUCTION OF THE STANDARD DISTANCE (30 CM.) TOGETHER WITH THE VARIABILITY.

(The latter given in each case immediately below the average reproduction.)

Type of Suggestion.	Averages for First 25 Judgments. Subject			Averages for Second 25 Judgments. Subject			Averages for Third 25 Judgments. Subject			Averages for Combined 75 Judgments. Subject		
	X.	*Y.*	*Z.*	*X.*	*Y.*	*Z.*	*X.*	*Y.*	*Z.*	*X.*	*Y.*	*Z.*
Visual — affirmative	30.88	30.4	33.64	28.66	30.50	30.38	27.12	30.70	30.86	28.886	30.533	31.626
	1.52	1.64	3.68	1.74	.86	1.04	2.88	.94	1.42	2.046	1.146	2.046
Visual — negative	30.34	30.08	33.56	28.30	31.04	30.48	26.82	30.84	31.08	28.486	30.653	31.706
	1.58	2.04	3.72	1.9	1.48	1.36	3.18	1.08	1.56	2.22	1.533	2.213
Vocal — affirmative	30.94	30.06	33.18	29.16	30.64	30.06	27.75	30.68	31.06	29.28	30.46	31.433
	1.34	1.34	3.3	1.36	1.12	1.10	2.32	1.08	1.54	1.673	1.18	1.98
Vocal — negative	31.10	30.58	33.40	28.382	30.90	30.50	23.74	31.02	31.46	27.78	30.833	31.786
	1.9	1.66	3.64	1.7	1.1	1.54	2.26	1.38	1.62	1.953	1.38	2.266
Auto.— affirmative	30.72	30.06	32.96	28.46	30.48	30.16	27.74	30.88	31.76	28.973	30.473	31.626
	1.76	1.02	3.28	1.66	.84	.88	2.3	1.12	1.76	1.906	.993	1.973
Auto. — negative	30.88	30.28	32.88	28.54	30.50	30.70	27.40	30.74	31.48	28.94	30.506	31.686
	1.6	1.6	3.12	1.62	1.54	1.50	2.6	1.14	1.96	1.94	1.426	2.193
No suggestion	31.24	29.76	32.66	29.32	30.52	30.24	28.16	30.72	31.28	29.573	30.333	31.393
	1.72	1.72	2.74	1.24	.92	1.64	2.04	1.0	1.52	1.666	1.213	1.966
[3]All types of suggestion combined	30.81	30.243	33.27	28.583	30.676	30.38	26.761	30.81	31.283	28.724	30.576	31.643
	1.616	1.55	3.456	1.663	1.156	1.236	2.59	1.123	1.643	1.956	1.276	2.112
Affirmative suggestions combined	30.846	30.173	33.26	28.76	30.533	30.20	27.54	30.753	31.226	29.048	30.488	31.561
	1.54	1.333	3.42	1.56	.94	1.0	2.5	1.046	1.57	1.875	1.106	2.0
Negative suggestions combined	30.773	30.313	33.28	28.406	30.813	30.56	25.98	30.866	31.34	28.388	30.664	31.726
	1.693	1.766	3.49	1.74	1.373	1.46	2.68	1.2	1.71	2.037	1.446	2.224
Visual suggestions combined	30.61	30.24	33.60	28.48	30.77	30.43	26.96	30.77	30.97	28.686	30.593	31.66
	1.55	1.84	3.70	1.82	1.17	1.2	3.03	1.01	1.49	2.133	1.34	2.13
Vocal suggestions combined	31.02	30.32	33.29	28.77	30.77	30.28	25.75	30.85	31.26	28.513	30.646	31.61
	1.62	1.5	3.47	1.53	1.11	1.32	2.29	1.23	1.58	1.813	1.28	2.12
Auto. suggestions combined	30.80	30.17	32.92	28.50	30.49	30.43	27.57	30.81	31.62	28.956	30.49	31.65
	1.68	1.31	3.20	1.64	1.18	1.19	2.45	1.13	1.86	1.923	1.21	2.083

[1] The 'constant error' is the difference between the standard distance (30 cm.) and the 'average reproduction'—the latter being the average of the actual estimates made. Thus the 'average reproduction' for the three estimates 33, 28 and 31 would be 30.67; the 'constant error' 0.67.

[2] 'Variability' (=the 'crude variable error') was obtained by adding the amount of variation from the standard (30 cm.), regardless of its sign, and dividing by the number of cases considered; thus the 'variability' for the three judgments 33, 28 and 31 would be 2.

[3] "All types of suggestion combined" excludes throughout estimates with no suggestion.

TABLE II.

AS REGARDS THE AVERAGE REPRODUCTIONS.

1. *Average Reproductions Compared with the Actual Distance Exposed.*

Subject.	First 25 Judgments.	Second 25 Judgments.	Third 25 Judgments.	Entire 75 Judgments.
X	Reprod. Dist. >	Reprod. Dist. <	Reprod. Dist. <	Reprod. Dist. <
Y	" " > (except 'no suggestion')	" " >	" " >	" " >
Z	Reprod. Dist. >	" " >	" " >	" " >

2. *Average Reproductions with no Suggestion Compared with Average Reproductions with Suggestion of any Kind.*

X	no sug. >	no sug. <	no sug. <	no sug. <
Y	" " < (except 3 affirm. cases and visual —)	" " < (except visual + and auto. + and —)	" " < (except visual + and vocal +)	" " <
Z	no sug. <	no sug. < (except vocal + auto. +)	no sug. < (except visual + vocal +)	" " <

3. *Average Reproductions with Affirmative Suggestion Compared with Average Reproduction with Negative Suggestion.*

X	vis. + > vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Y	vis. + > vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Z	vis. + > vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —

AS REGARDS VARIABILITY.

4. *Variability when there was no Suggestion Compared with that when there was Suggestion of any kind.*

Subject.	First 25 Judgments.	Second 25 Judgments.	Third 25 Judgments.	Entire 75 Judgments.
X	no sug. > (except vocal — and auto. +)	no sug. <	no sug. <	no sug. <
Y	no sug. >	no sug. < (except visual + and auto. +)	no sug. < (except visual +)	no sug. < (except visual +, vocal +, auto +)
Z	" " <	no sug. >	no sug. < (except visual +)	no sug. <

TABLE II. *Continued.*

5. *Variability when there was Affirmative Suggestion Compared with that when there was Negative Suggestion.*

X	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + > voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Y	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Z	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —

In the portion of Table I. which presents the averages for the three successive series of twenty-five judgments, as we might expect, this general contrast in the effect of affirmative and negative suggestions appears somewhat less regularly; yet the general result of combining the entire 75 judgments under any one type of suggestion is seen to be fairly well distributed over these smaller series.

Thus in the averages for the first series of twenty-five estimates seven out of the twenty-four groups showed a reversed effect for the affirmative and negative suggestions, that is, the negative had less average error or variability than the affirmative; five of these irregularities were as regards the error, the other two as regards the variability. In the second series of twenty-five estimates three of the twenty-four groups were irregular, one in respect to the error, the other two in respect to the variability. In the third series of twenty-five estimates, three irregularities out of the twenty-four groups again occurred, two as regards the error and one as regards the variability.

Accordingly, out of the seventy-two groups, when the judgments were considered in series of twenty-five, there were thirteen irregular groups. Of these thirteen irregularities seven were with the 'auto' type of suggestion, three were with the visual, two with the vocal, and one appeared in the comparison of 'affirmative suggestions combined' and 'negative suggestions combined,' in the first twenty-five estimates.

As regards the smaller constant error in the estimates made with no suggestion than with those where there was suggestion,

the results apparent in the combined seventy-five judgments appear also in each of the series of twenty-five judgments, save for subject X in the first series. As regards the smaller variability with no suggestion, exceptions occur in both the first and second series of twenty-five judgments, but none in the third series.

In view of the fact that the judgments made under suggestion whether affirmative or negative show so frequently an increase in variability and error beyond that in the judgments made without suggestion, we may infer that suggestion does in itself, and apart from the actual 'contents' of the suggestion, effect some change in the reproduction of distance.

But it is also significant that the suggestion acts to a considerable extent in a direction corresponding to the actual 'contents' of the suggestion given; *i. e.*, the error and variability under suggestions of *ability* were almost always less than when suggestions of *inability* were made.

Previous experimenters found that one subject differed from another radically in the degree and nature of the change which suggestion produced. This is true only within somewhat narrow limits, in the present experiments. The three subjects showed the same significant tendencies in responding to suggestion; that is, with each, the suggestions of ability produced generally less error and less variability than did the suggestions of inability; and estimates with no suggestion were in each case still nearer the standard. The only difference lay in the fact that with two of the subjects there was a constant tendency to lengthen, with the other subject to shorten, the reproductions of the standard interval of 30 cm. And this constant error, whatever its direction, was increased by suggestions, especially by those of 'inability.' It would be unjustifiable to say that one observer showed more susceptibility to suggestion than another merely upon the ground of a different direction or a different absolute amount in his departure from the standard. Rather the subjects should be compared upon a basis of the *change* in the amount or direction of the errors or variability according as suggestions were present or absent, or according as one form of suggestion or another was employed. Upon this basis (see Table III.) there appears some difference between the three subjects.

Table III.

Order of the Effectiveness of Suggestion.

Based upon seventy-five judgments for each type of suggestion.

I. As Measured by the Change in the Average Reproduction.

(The types of suggestion decrease in effectiveness from left to right.)

Subject.							
X	Voc. —	Vis. —	Vis. +	Auto. —	Auto. +	Voc. +	No suggestion
Y	Voc. —	Vis. —	Vis. +	Auto. —	Auto. +	Voc. +	No suggestion
Z	Voc. —	Vis. —	Auto. —	Auto. +	Vis. +	Voc. +	No suggestion

II. As Measured by the Change in Variability.

(The types of suggestion decrease in effectiveness from left to right.)

Subject.							
X	Vis. —	Vis. +	Voc. —	Auto. —	Auto. +	Voc. +	No suggestion
Y	Vis. —	Auto. —	Voc. —	No sug.	Voc. +	Vis. +	Auto. +
Z	Voc. —	Vis. —	Auto. —	Vis. +	Voc. +	Auto. +	No suggestion

Bell in his experiment notes that the effect of suggestion decreased with repetition. This seems to be only partly, if at all, true here. If the fact that suggestions of ability produce less error, and suggestions of inability greater error in the reproduction of distance attests anything as to the power of suggestion this experiment shows that the susceptibility more regularly occurs with repetition, since as the experiment proceeds there occur fewer groups where this effect is reversed. Table I. shows seven irregularities in this respect in the first series of twenty-five estimates, and three each in the second and the third series of twenty-five estimates. Apart from irregularity, the absolute amount of the difference between 'affirmative suggestions combined' and 'negative suggestions combined' shows in the case of Subject *X* a uniform *increase* in the successive series of twenty-five judgments; and the same is true of the difference between 'no suggestion' and 'all types of suggestion combined.' With the other subjects there is neither a uniform increase nor decrease.

The purpose of the experiment was only in part to investigate the effect of suggestion in general; to determine the relative influence of the different *types* of suggestion was equally the purpose.

In Tables III. and IV. is set forth the relative strength or effectiveness of these different types of suggestion. It there appears that for each of the subjects the negative suggestions

TABLE IV.

Type of Suggestions.	Ratios of Reproductions with Suggestion to Reproductions Without. Based upon the Constant Error in Reproduction and also upon the Variability; Latter in Parentheses. Subject			Ratios of Reproductions with Suggestion to the Actual Standard (30 cm.). Based upon the Error in Reproduction. Subject		
	X.	Y.	Z.	X.	Y.	Z.
All types of suggestion combined. (450 reproductions)	.971 (1.174)	1.008 (1.051)	1.007 (1.074)	.957	1.019	1.054
Affirmative suggestions combined. (225 reproductions)	.982 (1.125)	1.005 (.911)	1.005 (1.017)	.968	1.016	1.052
Negative suggestions combined. (225 reproductions)	.959 (1.222)	1.010 (1.192)	1.010 (1.131)	.946	1.022	1.057
Visual suggestions combined. (150 reproductions)	.970 (1.280)	1.008 (1.104)	1.008 (1.083)	.956	1.019	1.055
Vocal suggestions combined. (150 reproductions)	.964 (1.088)	1.010 (1.055)	1.006 (1.078)	.950	1.021	1.053
Auto. suggestions combined. (150 reproductions)	.979 (1.154)	1.005 (.997)	1.008 (1.059)	.965	1.016	1.055

(suggestions of inability) were about twice as effective as were the positive suggestions (suggestions of ability). Moreover for each subject the vocal negative suggestions were the most effective of all as measured by the constant change in the average reproduction. As measured by the variability, the visual negative suggestions were stronger. With all the subjects the vocal positive suggestions, on the other hand, had the least influence upon the average reproduction; while upon the variability the 'auto' positive type was among those having the weakest influence.

As suggestions of ability and inability appeared to bring about an increase in the constant error and in the variability the question was raised as to whether the results were due merely to the *distraction* which the suggestions produced, and not at all to their inherent 'content' or ideas. It hardly seemed that such a theory could be fully justified, for it would not account for the fact that different types of suggestion had regularly produced different effects — effects consistent with the 'content' of the suggestion. However, seventy-five additional tests were made on each of the three subjects, in which the pegs were exposed and the reproductions made while the observer counted aloud the strokes of a metronome, swinging at five different rates, in order that the counting might more likely act as a dis-

traction and not become a merely mechanical operation. The other conditions were identical with those in the tests made for suggestibility.

TABLE V.

RESULTS FOR ESTIMATES MADE UNDER DISTRACTION.

Subject.	Average Reproductions of the Standard Distance together with the Variability.			
	Averages for First 25 Judgments.	Averages for Second 25 Judgments.	Averages for Third 25 Judgments.	Averages for Combined 75 Judgments.
X	28.48	28.32	28.12	28.31
	1.68	1.64	1.88	1.73
Y	29.86	29.38	29.15	29.48
	.94	.86	.93	.89
Z	29.18	29.14	31.65	29.92
	1.02	1.54	1.88	1.46

The variability is thus seen to be, on the average, less than when suggestions were offered, and for the most of the subjects less even than in the cases where no suggestion was made. As for the average reproductions, the two subjects whose estimate of the distance had been constantly greater than the standard in the previous experiment now had a tendency to make it less than the standard. The other subject whose tendency had been to underestimate the standard interval now had a tendency to make it still shorter.

The fact that the results with distraction differed in these respects from the results with different types of suggestion indicates that at least with two of the three subjects the peculiar effect of these suggestions is not due solely nor even predominately to any distraction which may have inhered in them.

XII. The Effect of Various Types of Suggestion upon Muscular Activity.

BY EDW. K. STRONG, JR., M.S.

The purpose of the experiment here reported was to discover how far and in what manner maximum muscular activity may be affected by suggestions of various types. In all these the subject was instructed not to oppose any resistance to the suggestion nor on the other hand consciously to endeavor to carry out the suggestions, but his attention and conscious effort throughout was to be expended in exerting each time his maximum grip. Yet he was requested to maintain the suggestion in consciousness until after his muscular effort.

Historical Review.

There is scarcely any literature which bears directly upon the question under discussion in this paper. A number of articles[1] which have appeared lately indicate that some attention is being given the subject, but neither their methods nor their results have any direct relation to those of the present investigation. As far as I am aware Brand is the pioneer in this field of investigation. His experiment[2] was performed in this laboratory, and his purpose, as he states it, was "to find out how far and in what direction the visual estimation of a linear magnitude could be affected by suggestion of certain possible errors in such estimation, the subject knowing that the

[1] Triplet, 'Dynamogenic Factors in Pace-making and Competition,' *Amer. Jour. of Psych.*, IX., 507–533. Small, 'The Suggestibility of Children,' *Ped. Sem.*, IV., 182. Binet, 'La suggestibilité,' *L'année psych.*, V., 99. Pearce, 'Normal Motor Suggestibility,' PSYCH. REV., IX., 348. Smith and Sowton, 'Observations on Spatial Contrast and Confluence in Visual Perception,' *Brit. Jour. Psych.*, II., 196–219. A very full discussion of all but the first accompanies the article of Bell ('The Effect of Suggestion upon the Reproduction of Triangles and of Point Distances') in the *Amer. Jour. of Psych.*, XIX., 504.

[2] 'The Effect of Verbal Suggestion upon the Estimation of Linear Magnitudes,' PSYCH. REV., XII., 1905, 41–49.

suggestions were purely arbitrary, *i. e.*, that they had no reference to any foreseen tendency to err in any direction."[1]

The eight different suggestions used by Brand in his experiment were printed upon white cardboard in letters 1.2 cm. high. The conductor presented them to the subject by displaying the cardboard for a moment. After the suggestion had been presented the conductor displayed two small objects upon his frame and then called upon the subject to respond by setting up his two similar objects upon his own frame at a distance from each other approximating as nearly as possible that of the original objects.

I have reduced the results as given in his tables to percentages of the respective standards.

The following table gives the total results of Subject 'C,' eliminating the first group of experiments, since their variation is more than ten per cent. from the standard, due probably to lack of practice.

TABLE I.

Suggestion.	No. of Judgments.	Sum of Standards.	Result in per cent.
Zwp fjvic bgzx asye	49	1,258	100.5
Life is real where	49	1,258	99.5
Don't make too long	49	1,258	101.0
Don't make too short	49	1,258	101.0
Make short	104	2,632	99.5
Make long	104	2,632	102.0

And the following table gives the total results for Subject 'Y,' the first group of data having been likewise eliminated.

TABLE II.

Suggestion.	No. of Judgments.	Sum of Standards.	Result in per cent.
Zwp fjvic bgzx asye	49	1,086	91.0
Life is real where	49	1,086	90.0
Don't make too long	84	1,810	92.0
Don't make too short	84	1,810	92.5
Make short	100	2,400	94.0
Make long	100	2,400	94.0

[1] It is just at this point that suggestions as experimented upon in Bell's (see page 5) and Brand's work and in my own differ from the others. As Bell puts it: "The most potent factor without doubt in cases of suggestion is the arousal of an attitude of general expectancy." This factor has been utilized in the experiments of others, but is so far as possible eliminated in these three, for here the subject was particularly instructed that he was not to respond consciously to the suggestion but to endeavor consciously to exert his maximum grip.

From these tables I do not feel that any clear-cut deductions can be made, with the possible exception that the two brief suggestions 'Make short' and 'Make long' tend more than the other suggestions to make the reproduced distance greater in magnitude, and the two suggestions 'Don't make too long' and 'Don't make too short' tend to a less degree to have the same effect.

In Table III. Brand has brought together from group D (or the last group of experiments with both subjects) the totals for all the suggestions containing the word 'long' in one column and all those containing the word 'short' in another column, for comparison. The percentages as given below were inserted by myself.

TABLE III.

Subject.	No. of Judgments.	Sum of Standards.	'Long.'	Per cent.	'Short.'	Per cent.
'C'	58	1,628	1,702.3	104.5	1,644.4	101.0
'Y'	36	580	556.3	95.5	542.6	93.5

If compiled from all four of the groups instead of group *D* alone we have the following.

TABLE IV.

Subject.	No. of Judgments.	Sum of Standards.	'Long.'	Per cent.	'Short.'	Per cent.
'C'	207	4,932	5,095.8	103.0	5,052.2	102.5
'Y'	189	4,294	4,008.2	93.8	4,013.2	93.9

Here we have no superiority of the 'long' mottoes over the 'short' mottoes, except with the one subject.

Let us now turn to the second part of his experiment. Here but three suggestions were used: 'long,' 'short' and 'XXXX.' The latter was used as a check or neutral. Quoting from him: "Four subjects were employed and the results were not very uniform, two of the subjects showing no decided tendency towards anything resembling a constant effect, while the other two subjects showed a clear general constancy of considerable difference throughout."

In conclusion it seems to me that one cannot point with any emphasis to any direct effect from the above varied suggestions upon estimation of distance except (1) that 'long' suggestions may consistently affect one person more than corresponding

'short' ones; (2) that the reverse effect may be produced upon a second person; and (3) that there may be no appreciable effect of one or the other upon a third person.

In Bell's experiment[1] two types of suggestion were used — the auditory and the visual. For the former such as 'make high,' 'make low,' 'make high enough,' etc., or simply 'high' and 'low' were used, being spoken by the conductor just before the presentation of the object to be reacted upon. For the visual suggestion a diamond-shaped figure 20 cm. long and 4 cm. wide was shown to the subject. When displayed in a vertical position it was thought that it might serve as a 'high' suggestion, being much taller than any other figure shown. Likewise when shown in a horizontal position it was called a 'low' suggestion. (As far as I am aware the subject was not informed what this visual suggestion was intended to suggest. It may not then have had an equal effect upon the different subjects.)

The forms chosen for reproduction were: (1) triangles of different shapes and heights and (2) vertical point distances as shown by (*a*) a dot above the center of a base line and (*b*) a dot above another dot.

Under the first group (triangles of different shapes and heights) ten triangles were used, of the same base (10 mm.), but of different heights and shapes, and varying from 49 to 100 mm. in altitude. The work was so carried on that nine reproductions of each triangle were made with each kind of suggestion (*i. e.*, auditory and visual). Three of the nine were reproduced with 'high' suggestions, three with 'low' suggestions, and three without any suggestion. The altitude of the reproduced triangle was compared wiih the standard, and the difference expressed in millimeters as a positive or negative error. But the weakness of his experiment lies in the small number of observations per subject per triangle per suggestion, *i. e.*, *three* in number. Averages drawn from so few observations are hardly of greatest weight.

Bell concludes "that in general the suggestions do affect the reproduction of the triangles; that the auditory suggestion

[1] 'The Effect of Suggestion upon the Reproduction of Triangles and of Point Distances,' *Amer. Jour. of Psych.*, XIX., 1908, 504.

is more effective than the visual; and that in the auditory set the 'low' suggestion is more effective than the 'high.'" Upon looking at the subjects individually we find they have reacted differently. Subject 'B' alone showed striking susceptibility to the suggestion in all the cases. Subjects 'A,' 'C' and 'D' with the auditory suggestions and Subject 'E' with the visual suggestions, estimated the apex of the triangles below the standard when given either 'high' or 'low' suggestions. Throughout the experiment susceptibility to 'low' suggestions was more general and more uniform than to 'high' suggestions.

Under the second group (vertical point distances) the dot-above-line experiment was completed with auditory suggestions and the dot-above-dot experiment with visual suggestions. Bell in commenting upon his results states that there was little indication that the suggestions had any constant effect: with subjects 'C' and 'D' the 'high' and 'low' suggestions are both lower than the standard, the 'low' the lowest; while with subjects 'E' and 'F' the 'high' and 'low' suggestions are both higher than the standard.

Description of the Present Experiment.

The series of experiments carried on as described below can be divided into two parts, the first part consisting of those experiments which were performed during the months from September to December, 1908, and the second part consisting of those which were performed during the months from January to April, 1909.

The general plan of the experiment was to give the subject a suggestion, and then have him respond each time with his maximum grip. Collin's elliptical form of dynamometer was used and from it an expression in kilograms was obtained of his muscular activity.

The subject was seated in a chair which was provided with two flat arms about two and a half inches wide which extended as far forward as the front edge of the seat. The subject rested his arms from the elbow to the wrist upon these arms of the chair. When actually gripping the dynamometer the palm of his hand was uppermost. In the intervals he was allowed to

rest his hand as he chose. The conductor sat at a small table. On the edge of this table between the subject and the conductor a large upright black screen was placed in such a manner that the subject could not see anything upon the table; nor could he see the conductor.

Once every twenty seconds, until the series with one hand was completed, the subject gripped the dynamometer with his maximum force. The exact procedure was as follows. When the second-hand of the watch indicated the moment of presentation, the suggestion was given by the conductor. The dynamometer was then placed in the subject's hand. As soon as the latter had responded with his maximum grip the conductor took the instrument from his hand, noted the reading, and then awaited the next moment of presentation.

In all there were seven suggestions offered. They might be classified as follows:

I. *Auditory Suggestions.*

1. Positive. "Now you can make it stronger than usual."

2. Negative. "Now you can't make it as strong as usual."

These suggestions were presented vocally by the conductor.

II. *Visual Suggestions.*

1. Positive. A plus (+) sign 0.4 inches in size displayed upon a piece of cardboard two inches square.

2. Negative. A minus (−) sign 0.4 inches in size displayed upon a piece of cardboard two inches square.

In giving these suggestions the conductor placed the cardboard upon the corner of the table in front of the subject.

At the commencement of the experiment each subject was told that the plus (+) sign was meant to suggest to him that he could make his grip stronger than usual and the minus (−) sign was meant to suggest that he could not make it as strong as usual. These signs were consequently visual suggestions depending on previous vocal instruction.

III. *Auto-suggestions.* Here the conductor announced in the same tone and manner as in presenting the Auditory Suggestions "Now you can make a suggestion of your own." The subject understood by this that he was at liberty to suggest to himself either the positive or negative suggestion and to designate

his choice to the conductor by audibly announcing it. In this case as soon as the subject had announced his suggestion the dynamometer was handed him and the experiments continued as usual.

1. Positive. (After suggestion by the conductor, as above.) "Now I can make it stronger than usual," spoken by the subject.

2. Negative. (After suggestion by the conductor, as above.) "Now I can't make it as strong as usual," spoken by the subject.

IV. *Neutral 'Suggestion.'* This consisted of the announcement by the conductor of, 'Now, neutral,' and was intended to act merely as a check and guide to what would be the exertion if no suggestion of any sort were given.

In the earlier experiments a series consisted of fifty-six experiments or readings, twenty-eight with each hand. These twenty-eight in turn consisted of four each of the seven different suggestions. These twenty-eight were presented to the subject in a haphazard arrangement previously determined upon. All subjects were given the same haphazard arrangements and in the same order, so that direct comparisons between them could be made. The right hand in every case was experimented upon first. After four days' work with each subject, ten neutrals instead of four were introduced, thus making a day's work or series consist of thirty-four experiments instead of twenty-eight. Besides these twenty-eight or thirty-four experiments there were always two extra neutrals at the start which were for the purpose of practice and were always discarded.

A day's work with subject 'B' consisted of two series during one hour, — that is to say, of fifty-six experiments with each hand taken in this manner: twenty-eight with the right hand, then twenty-eight with the left hand, then twenty-eight with the right, and finally twenty-eight more with the left. A day's work with subjects 'J' and 'W' consisted of only one series during one hour. During the second term but one series per day was taken with any one of the three subjects. It should also be stated here that all three subjects were familiar with experimental work.

The procedure during the *second term* was exactly the same as in the first term except for the following. Instead of seven suggestions nine were employed. The two auto-suggestions were omitted and the following four added. Two auditory suggestions consisting of 'Now, plus' and 'Now, minus' to correspond to the two visual suggestions employed during the first term. Also two visual suggestions consisting of the mottoes: "Now you can make it stronger than usual" and "Now you can't make it as strong as usual" printed on cards 5.5 × 1.25 inches. These visual suggestions were to correspond to the auditory suggestions employed during the first term. Moreover the visual suggestions (+) and (−) were presented on cards similar to those just described, instead of on cards two inches square. Instead of 28 or 34 experiments on each hand per day there were 42 such experiments. These 42 consisted of four each of the eight suggestions and ten of the neutrals, arranged as before in a haphazard manner. During the second term the right hand was not experimented upon first each day, but only in alternation with the left hand.

The nine suggestions of the second term and the two auto-suggestions of the first term will be referred to in the tables by the following set of symbols.

I. *Auditory Suggestions* — in quotation marks.
   'Can' means "Now you can make it stronger than usual."
   'Can't' means "Now you can't make it as strong as usual."
   '+' means "Now plus."
   '−' means "Now minus."

II. *Visual Suggestions* — in brackets.
   (Can) means the motto, "Now you can make it stronger than usual" printed on a card.
   (Can't) means the motto, "Now you can't make it as strong as usual" printed on a card.
   (+) means the symbol '+' printed on a card.
   (−) means the symbol '−' printed on a card.

III. *Auto-suggestions.*
   + Auto means "Now I can make it stronger than usual," spoken by the subject.
   − Auto means "Now I can't make it as strong as usual," spoken by the subject.

IV. *Neutral 'Suggestion.'*

'N' means "Now, neutral."

The Results.

In the following two tables there are presented the averages of 48 experiments for each type of suggestion with either hand of the three subjects, the numbers indicating in kilograms the maximal grip.

Table V.

Results for the First Term.

The numbers for the first six suggestions under Subjects 'B' and 'J' are an average of 48 results, and under Subject 'W' are an average of 24 results. The neutrals under Subjects 'B' and 'J' are an average of 96 results while under Subject 'W' they are an average of 36 results.

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	38.8	29.6	17.2	15.0	33.1	23.7
'Can't'	38.4	28.9	17.1	15.4	33.3	23.4
(+)	38.9	29.9	17.7	15.0	33.8	24.4
(−)	38.6	29.3	17.1	14.8	33.5	24.4
+Auto.	39.7	29.5	17.5	15.2	33.5	24.2
−Auto.	39.0	29.9	17.1	15.3	33.6	25.6
Average	38.9	29.5	17.2	15.1	33.5	24.3
Neutral	39.0	29.4	16.9	15.0	33.4	23.5
Average of all	38.9	29.5	17.1	15.1	33.5	23.9

Table VI.

Results for the Second Term.

The numbers for the first eight suggestions are an average of 48 trials while those for the neutrals are an average of 120 trials.

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	40.3	31.8	16.9	15.9	32.8	25.1
'Can't'	39.7	31.4	16.9	16.3	32.6	25.4
(Can)	39.4	31.0	16.9	16.4	32.9	25.3
(Can't)	40.1	31.3	16.8	15.9	33.2	25.3
'+'	39.6	31.2	16.7	16.2	32.2	24.8
'−'	39.4	30.8	16.7	16.1	32.1	25.1
(+)	39.3	31.3	16.9	16.3	32.4	25.3
(−)	39.6	31.3	16.7	16.4	32.8	25.9
Average	39.7	31.3	16.8	16.2	32.6	25.3
Neutral	39.4	30.8	16.7	16.0	32.2	24.9
Average of all	39.5	31.1	16.8	16.1	32.5	25.2

From these two tables we see very clearly that suggestion as a whole heightens the maxima. In every case, except for the right hand of subject 'B' during the first term, the average of the neutrals for any of the three subjects with either hand is less than the corresponding average of all the other suggestions. The exception can probably be explained by an introspection of subject 'B' on March 1, when he stated that neutrals at *first* had a positive effect, that is to say, he felt that then was a time to make a 'record.'

In the following two tables we have a statement of the mean variation of the quantities entering into the preceding tables: Table VII stating the mean variation of the quantities in Table V, and Table VIII stating those of Table VI.

MEAN VARIATION TABLE VII. (FIRST TERM.)

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	1.88	2.00	.86	.96	3.17	1.68
'Can't'	2.34	1.70	.74	.87	2.77	1.48
(+)	2.59	1.89	1.11	1.09	2.46	1.71
(−)	2.29	1.84	1.04	.83	2.62	1.70
+ Auto.	2.21	2.32	.93	.81	2.30	1.61
− Auto.	1.74	1.86	.93	.88	2.26	1.98
Average	2.17	1.93	.93	.91	2.59	1.69
Neutral	1.94	1.57	.85	.94	2.99	2.08
Average of all	2.14	1.88	.92	.91	2.65	1.75

MEAN VARIATION TABLE VIII. (SECOND TERM.)

Suggestions.	Subject 'B'		Subject 'J'		Subject 'W'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	2.58	1.54	.63	1.03	2.17	2.32
'Can't'	2.57	1.88	.73	1.01	2.62	2.31
(Can)	2.04	.80	.76	.91	2.06	2.19
(Can't)	1.80	.67	.67	1.02	2.38	2.39
'+'	1.82	.80	.79	1.08	2.32	2.24
'−'	2.17	.80	.77	1.13	2.61	1.97
(+)	2.46	.87	.81	1.20	2.11	2.04
(−)	1.97	.92	.89	1.13	2.11	2.06
Average	2.18	1.03	.76	1.06	2.29	2.19
Neutral	2.05	1.69	.85	1.12	2.37	2.50
Average of all	2.16	1.11	.77	1.07	2.31	2.22

In Table IX. we have a restatement of Table V., and in Table X. restatement of Table VI. to show the effect respectively of positive, negative and neutral suggestions upon the three subjects.

From these tables it is evident that the negative suggestions tend more than the positive suggestions with subject 'W' to heighten the maxima, and this is especially true with his left hand. But with the other two subjects the positive suggestions as a general rule are superior to the negative in this respect. However in all three cases the negative auto-suggestions with the left hand are clearly superior to the positive in heightening the maxima. Why this is the case is difficult to say. Possibly, the subject feels that after audibly declaring that he can't make it as strong as usual, he must exert greater effort in order to neutralize the suggestion's effect and actually obtain his maximum. With the right hand, however, this tendency does not appear, perhaps because of the hand's greater use and its consequent greater fineness in discrimination. Indeed it appears

TABLE IX.

SUBJECT 'B.'

	With the Right Hand.			With the Left Hand.		
	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory	38.8	38.4		29.6	28.9	
Visual	38.9	38.6		29.9	29.3	
Auto.	39.7	39.0		29.5	29.9	
Neutral			39.0			29.4
Average	39.1	38.7	39.0	29.7	29.4	29.4
SUBJECT 'J.'						
Auditory	17.2	17.1		15.0	15.4	
Visual	17.1	17.1		15.0	14.8	
Auto.	17.5	17.1		15.2	15.3	
Neutral			16.9			15.0
Average	17.3	17.1	16.9	15.1	15.2	15.0
SUBJECT 'W.'						
Auditory	33.1	33.3		23.7	23.4	
Visual	33.8	33.5		24.4	24.4	
Auto.	33.5	33.6		24.2	25.6	
Neutral			33.4			23.5
Average	33.5	33.5	33.4	24.1	24.5	23.5

TABLE X.

	With the Right Hand.			With the Left Hand.		
	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
SUBJECT 'B.'						
Auditory can–can't	40.3	39.7		31.8	31.4	
Visual can–can't	39.4	40.1		31.0	31.3	
Auditory + and —	39.6	39.4		31.2	30.8	
Visual + and —	39.3	39.6		31.3	31.3	
Neutral			39.4			30.8
Average	39.7	39.7	39.4	31.3	31.2	30.8
SUBJECT 'J.'						
Auditory can–can't	16.9	16.9		15.9	16.3	
Visual can–can't	16.9	16.8		16.4	15.9	
Auditory + and —	16.7	16.7		16.2	16.1	
Visual + and —	16.9	16.7		16.3	16.4	
Neutral			16.7			16.0
Average	16.9	16.8	16.7	16.2	16.1	16.0
SUBJECT 'W.'						
Auditory can–can't	32.8	32.6		25.1	25.4	
Visual can–can't	32.9	33.2		25.3	25.3	
Auditory + and —	32.2	32.1		24.8	25.1	
Visual + and —	32.4	32.8		25.3	25.9	
Neutral			32.2			24.9
Average	32.6	32.7	32.2	25.1	25.5	24.9

true that the so-called maximum effort in this experiment is not a real maximum exertion each time, but rather is an effort to attain a sort of definite standard. The left hand is thus at a disadvantage in maintaining its maximum, or definite standard, because of its lesser use and inferior discrimination.

In Tables XI. and XII. we have still a different restatement of Tables V. and VI. for the purpose of showing the effect of particular types of suggestion. In these tables the figure placed opposite each suggestion is the average of that type's positive and negative suggestions taken together; and the types are arranged according to the magnitudes of the averages.

We must conclude from these tables that: (1) the auto-suggestions tend most strongly of all the types of suggestion to heighten the maxima; (2) that during the first term the visual suggestions were superior in this respect to the auditory suggestions with subjects 'B' and 'W,' and were inferior with subject

TABLE XI.

WITH THE RIGHT HAND.

Subject 'B.'		Subject 'J.'		Subject 'W.'	
Auto.	39.35	Auto.	17.3	Visual	33.65
Neutral	39.0	Auditory	17.15	Auto.	33.55
Visual	38.75	Visual	17.1	Neutral	33.4
Auditory	38.6	Neutral	16.9	Auditory	33.2

WITH THE LEFT HAND.

Auto.	29.7	Auto.	15.25	Auto.	24.9
Visual	29.6	Auditory	15.2	Visual	24.4
Neutral	29.4	Neutral	15.0	Auditory	23.55
Auditory	29.25	Visual	14.9	Neutral	23.5

TABLE XII.

WITH THE RIGHT HAND.

Subject 'B.'		Subject 'J.'		Subject 'W.'	
Auditory Can–Can't	40.0	Auditory Can–Can't	16.9	Visual Can–Can't	33.1
Visual Can–Can't	39.8	Visual Can–Can't	16.9	Auditory Can–Can't	32.7
Auditory + and —	39.5	Auditory + and —	16.8	Visual + and —	32.6
Visual + and —	39.4	Visual + and —	16.8	Neutral	32.2
Neutral	39.4	Neutral	16.7	Auditory + and —	32.1

WITH THE LEFT HAND.

Auditory Can–Can't	31.6	Visual + and —	16.4	Visual + and —	25.6
Visual + and —	31.3	Visual Can–Can't	16.2	Visual Can–Can't	25.3
Visual Can–Can't	31.2	Auditory + and —	16.2	Auditory Can–Can't	25.2
Auditory + and —	31.0	Auditory Can–Can't	16.1	Auditory + and —	25.0
Neutral	30.8	Neutral	16.0	Neutral	24.9

'J,' while during the second term the visual suggestions were superior with subject 'W' and the left hand of subject 'J', and were inferior with the right hand of subjects 'B' and 'J'; and (3) that during the second term the motto suggestions (*e. g.*, "Now you can make it stronger than usual") were superior to the symbol suggestions (*e. g.*, plus sign) in heightening the maxima.

Before attempting any explanation of these results it should be borne in mind that Miss G. M. Jones, working at the same general problem outlined in this paper, and with two of the same subjects, but upon the reproduction of distance instead of upon maximal exertion, obtains the most accurate reproduction of distance with the neutral 'suggestion,' while the other suggestions,

instead of aiding in the reproduction of distance, apparently disturb the accuracy of such reproduction. (It is notable that all three subjects were affected nearly alike in her experiment while there were such different effects with different subjects in my own experiment, as well as in Brand's.)

Suggestion then affects the reproduction of distance by acting as a disturbing factor, but aids, as my results show, when applied to maximal muscular effort. From these two experiments it seems probable that when *accurate* work is to be done all suggestions prevent the best work; but when mere *amount* of muscular effort is called for, any arousal of the attention to the work acts as a stimulant and enables the person to do better than he would otherwise have done. This effect is also shown by an incident which occurred with subject 'W.' Near the close of the series with his left hand the experiments were interrupted by a messenger who notified him that he was awaited elsewhere. The eagerness to be through the experiment caused a rise of approximately five kilograms, or eleven pounds, in his grip for the remaining eight trials.

I have used the word 'attention' in this connection because I do not know a better, yet I am not entirely satisfied that it is a question of the attention, *i. e.*, that it is an intellectual arousal that causes the results described in this paper. That there is an arousal of the whole person is certain; but whether the heightened maxima, as in the case just cited, are due to the attention directly or to an indirect effect of the general stir, it is difficult to be certain. I do not feel that it is a question of the will primarily, for that factor is supposed to be eliminated from the experiment by the instruction at the commencement of work, when the subject was told to make his maximum effort each time.

Yet with these misgivings, it seems natural to explain the greater efficacy of the auto-suggestions by a greater concentration of the attention upon the work in hand. An auto-suggestion consisted of the statement by the conductor of the experiment, "Now you can make a suggestion of your own"; this was followed by the statement of the subject, "Now I can make it stronger than usual" or "Now I can't make it as strong as

usual." The whole process tended to call forth greater concentration of attention to the next grip of the dynamometer than would result from either the auditory or visual suggestion, where there was only an act on the part of the conductor.

On the other hand a certain broad arousal perhaps accounts for the marked superiority of the motto suggestions, "Now you can make it stronger than usual" and "Now you can't make it as strong as usual" over the symbol suggestions, '+' and '—.' The latter, especially when spoken ('auditory suggestion') occupied but a moment and did not arouse the subject as did the motto-suggestions, for the latter required a greater length of time to be understood.

Introspections of all three subjects were in general that the suggestions had little effect upon them. For example at the end of the second term subject 'B' once stated that the auditory suggestion 'can' seemed perhaps most powerful and the visual suggestion (+) was next, but that it did not seem as if any were strong enough to produce an effect. And about the same time subject 'J' stated her feeling as follows, "When I stop to think the suggestion seems to have little effect, but when not thinking I feel that I obey the suggestion." Throughout the experiment notes were taken of those cases in which the subject expressed himself as satisfied or dissatisfied with the result. A careful analysis of these fails to show any correlation between the actual result and the introspections.

XIII. The Localization of Diasclerotic Light.

BY G. M. STRATTON.

Some time ago Veraguth[1] announced that the sensation produced by stimulating the retina through the sclerotic coat instead of through the pupil is often localized upon the same side as that upon which the stimulus falls, and that this is particularly true when the stimulus falls upon the temporal side. With some persons, however, he found that under these circumstances there was, in addition to the sensation on the temporal side, a weaker sensation localized upon the nasal side. On the other hand, when the stimulus was applied to the nasal side the sensation was always localized in the normal way, that is, it was referred to the temporal side. When the light was applied to the sclerotic, not in a radial, or diametric, direction, but in a direction approximating that of the tangent at that point, no change was noticed in the place the light seemed to occupy in the visual field. From this fact Veraguth argues against the assumption that the light, when it falls upon the temporal side of the sclerotic, passes through and strikes the retina upon the nasal side and therefore is referred in quite normal fashion to the side opposite to that upon which it really affects the retina. He believes that we have here an indubitable departure from the common cross-localization of visual impressions, and would explain this departure by the biological principle of utility. As regards the need of a correct localization of light penetrating the sclerotic, there is, he feels, a great difference between the temporal side of the eye which is open and exposed, and the inner or nasal side which is screened by the nose. And he maintains that the correct orientation of light coming diasclerotically is important only when the light falls on the *temporal* side; on the nasal side the light would come not only through the sclerotic coat, but through the pupil, and therefore would

[1] 'Die Verlegung diaskleral in das menschliche Auge einfallender Lichtreize in den Raum,' *Zeitschrift für Psychologie*, Vol. 42, pp. 162 ff.

be localized correctly according to the well-known law. Pressure phosphenes, unlike many of these sensations produced by diasclerotic stimulation, are all projected, he finds, to the opposite side of the visual field; and this to him seems reasonable, because there is no advantage in their being projected otherwise. They therefore follow the general law for the projection of a stimulus which comes through the pupil.

Veraguth believes that the present phenomena are a stumbling block in the way of the nativistic theory of vision which supposes that the space value of the impression inheres in the retinal element. For, if this were the case, why should there be a difference between the localization of the sensation resulting from the diasclerotic stimulation and that from the diapupillary? But the empiristic theory, he argues, can well admit and explain these diverse facts, for it supposes that localization is due to a complex of factors, of which the retinal factor is but one. Now the diasclerotic and the diapupillary stimulation, he holds, may each arouse a different group of factors to determine its localization. Each may well be connected, for example, with a different group of muscular responses, and therefore, according to the empiristic view, have a different localization. With some persons it is not improbable that both the complex of factors concerned in diasclerotic localization and the complex concerned in diapupillary localization may come into play simultaneously, and thus there be brought to pass by diasclerotic stimulation a localization at once on both sides of the visual field. Thus he would explain the double projection which was sometimes noticed in his experiments.

To control Veraguth's data and explanation a number of experiments were tried by the present writer, at first by means of a very strong beam of artificial light in a dark-room and brought to a point on the sclerotic coat by means of a system of screens and lenses. Afterwards, a device essentially the same as that employed by Veraguth himself[1] was adopted. It consisted of a portable flash-light so covered at the end that the light was emitted only from a circular area less than one millimeter in diameter.

[1] Veraguth, "Zur Prüfung der Lichtreaction der Pupillen," *Neurologisches Centralblatt* (16 April, 1905), XXIV. Jahrg., 338 ff.

This small opening lying well forward and beyond the body of the lamp could be brought close to the sclerotic coat of the eye, yet without actual contact. In this way the disturbing sensations, as well as the reflexes so difficult to suppress, were avoided. The observations were made by four persons; by two of these during frequent repetitions of the experiment over a considerable stretch of time. In general, these experiments while confirming much that Veraguth himself reports, yet bring some modification and supplement of his data, and lead, I venture to suggest, to a different conclusion.

There are regions upon the temporal side where the light is localized in the normal way; that is, upon the nasal side. There are regions where the light is localized entirely upon the temporal side. There is often found also a zone where there is some kind of localization upon both sides at once. The relation of these zones to one another is as follows: The region where the sensations are localized on the same side begins immediately posterior to the outer edge of the iris and extends backward a distance nearly equal to the distance from the center of the pupil to the outer edge of the iris. The region where the localization is upon the opposite side is still farther back. The region of double localization lies at the junction, or between the borders, of these two zones.

Upon the nasal side of the eye my own observations confirm, but not entirely, those of Veraguth. The localization is now far more frequently upon the side opposite to that upon which the stimulus falls, than in the case of temporal stimulation through the sclerotic coat. Yet occasionally there are found localities well forward where the sensation is localized upon the same side as the stimulus.

The localization of pressure phosphenes shows this peculiarity: that the phosphenes, so far as I can observe, are obtainable only upon those regions of the eye where light stimulus, passing through the sclerotic coat, arouses what I should call a more definite, a more figurate or punctiform sensation. In the anterior region where the application of light upon the sclerotic gives a vague sensation of light, local pressure upon the sclerotic coat produces no phosphenes whatever. The phosphenes,

however, when produced, are always localized upon the side opposite to that of the incidence of the stimulus, as Veraguth himself observed. By test experiments in which there was carefully noted the angle in the visual field where the extreme outlying phosphenes are localized, I find them ceasing to appear at about the outmost limit for the perception of light coming through the pupil. This would tend to confirm one in the belief that the retinal limit for response to actual light is the same as that for pressure stimulation.

Veraguth has laid considerable emphasis upon the fact that no change in the localization of the sensation takes place when, instead of having the stimulus come to the outer coat of the eye in a diametric direction, it is introduced in a direction approximately that of the tangent at that point. It does not seem, however, that this fact should be regarded as of great importance. It *would* be of importance were the outer coats of the eye perfectly transparent; but the sclerotic, being translucent, would in any event diffuse the light much as would tissue-paper or egg-shell; that is to say, the light would be transmitted in all directions from the point of incidence more or less indifferently, whether the light itself, in arriving at that point, came in one direction or another. In consequence, we should expect that if the sensation itself were in anywise due to the passage of light into the bulb of the eye, and over to the retina upon the opposite side, it would not be affected by a change from diametric to tangential incidence.

Another fact which must be taken into account in the explanation of the experimental data is that there is a retinal zone of considerable width anterior to the *ora serrata* which contains neither rods nor cones.[1] Yet this is a region where light, falling upon the sclerotic coat, nevertheless produces light sensations. And, furthermore, the present experiments lead me to believe that this is the region especially liable to the production of the diffused sensations which are localized upon the temporal side, both by temporal and by nasal stimulation. It would seem, to

[1] See Piersol, *Human Anatomy*, 1907, pp. 1456, 1467; Tolot, *Anatomischer Atlas*, 1907, p. 892; Werner Spalteholtz, *Hand Atlas of Human Anatomy*, tr. Barker, III., 772, 780; Huber, *Text-book of Histology*, tr. Cushing, 1900, p. 422. I am indebted to my colleague, Dr. Moody, for assistance upon this point

express it mildly, anomalous to explain the localization of light here as though there were actual light-sensitive elements directly beneath the region of the sclerotic where the stimulus falls.

There is, one must confess, some difficulty at the present time in giving any explanation of all the facts which appear in this interesting experiment. Yet on the whole there would seem sufficient reason to believe that Veraguth's theory is forced and improbable.

In the first place, it is hardly in keeping with other well-known facts of retinal behavior to suppose that the stimulation of the very same retinal elements will lead to such different localization by reason of some change in the manner of *approach* to those elements. The stimulation of the rods and cones by pressure, for example, — a stimulation which also comes through the sclerotic, and consequently by a course entirely different from that of ordinary photic stimulation — occasions no upset of the normal localization of these elements; their sensation is referred to exactly the same place as though it were caused by light, and had come through the pupil.

The facts perhaps are better explained by assuming that the light which penetrates the sclerotic coat *posterior* to the *ora serrata* and which consequently excites the light-sensitive elements of the retina directly beneath or adjacent produces the definite or figurate sensation which is localized upon the opposite side. This is the region where the localization of phosphenes and that of sensations produced by light stimulus are identical. But where the light falls *anterior* to the *ora serrata* it of course cannot reach immediately and upon the same side of the eye light-sensitive elements but can stimulate them only after first being diffused through the interior of the bulb. The character of this diffusion will be such that the light will pass in straight lines from its point of incidence upon the sclerotic, but in lines traversing the interior of the bulb in *all* directions and not just in the direction which is a continuation of that along which it was travelling before its incidence. The behavior of the light here is such as one would get in a dark room if a beam of light fell upon a plate of ground-glass in an aperture of the

wall. There would be a general illumination of the room but naturally more intense upon the side opposite to the illuminated spot, yet diffused and unfigurate upon that side.

This would account for two facts: namely, that the sensation in the case of such light stimulation as falls very far forward on the sclerotic is exceedingly indefinite, and is localized upon the same side as that upon which the light itself has been introduced into the eye. Furthermore, it would account for the fact, already referred to, that a change in the direction along which the light falls upon the sclerotic does not alter the character of the projection, provided the point of incidence remain unchanged. The position of greatest illumination in our dark-room would not be altered perceptibly by altering the direction along which the light proceeded to the plate of ground-glass.

The behavior of the sensation produced by light falling on the nasal side one must confess is mildly puzzling. For the most part all stimuli here, no matter how far forward they fall, are referred to the opposite side; yet we should expect that if we go forward of the *ora serrata* on this side we should find that the sensations were referred exclusively to the nasal side. The facts here obtained may perhaps be accounted for by the fact that the *ora serrata* upon this side reaches farther forward [1] than upon the opposite side, in accord with the familiar observation that the field of view upon the temporal side is always wider than upon the nasal. Consequently the stimulus introduced diasclerotically upon the nasal side would almost inevitably find light-sensitive elements adjacent to, if not directly beneath, the point of incidence; and the sensation thereby produced would by its intensity quite drown any sensation arising from a diffusion inside the eye. It is possible also that this phenomenon of irregular localization is to some extent occasioned by a reflection of light from the surface of the crystalline lens perhaps near its periphery. This surface of the lens might easily reflect the light back to the same side as that upon which it entered the sclerotic, though to a point somewhat farther back. Thus it would reach the sensitive portions of the retina and now quite normally be localized upon the opposite side.

[1] See, *e. g.*, Werner Spalteholtz, *op. cit.*, III., p. 780.

All cases where there is a double localization from a single stimulation, that is to say, a localization at once upon both sides of the visual field, could well be accounted for by supposing that the light, after passing through the sclerotic, reaches not solely those light-sensitive elements which lie in the portions of the retina immediately beneath or adjacent, but is also diffused to the opposite side of the inner chamber and there falling diffusely upon the retina is referred to the side opposite to that upon which the more definite sensation is localized. We should thus have two spacially disjoined sensations occurring from a single impression because the stimulus actually fell upon disjoined portions of the retina.

My own feeling therefore is that nothing appears in these experiments with diasclerotic light which is at all discordant with the law governing the localization of diapupillary impressions. Visual localization is based upon the positions which objects normally occupy when stimulating the different points of the retina; and this localization, once established, takes no account whatever of the course by which the stimulus happens actually to arrive. No questions are asked as to whether it has entered by the door or has broken in as a thief and a robber; it is treated in all cases psychically alike.